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STRIFE ABOUT COMPLEMENTARITY (I) *

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UNTIL a few years ago only a few physicists questioned the usual interpretation of quantum theory. Their criticisms were doubtless useful, but remained mainly on the philosophical level; no consistent alternative interpretation of the successful mathematical formalism was offered. Now the situation has altered substantially: several realistic, rational, and deterministic interpretations of the same formalism have been advanced. As was to be expected, they are strongly opposed by the upholders of the official philosophy of quantum theory, which is essentially of a positivistic character. The purpose of the present paper is to examine a recent manifestation of this conservative standpoint, namely, the article in which Professor L. Rosenfeld¹ of Manchester, who is Bohr's best known pupil, criticises the new realistic, rationalistic, and deterministic trends.

1 *What is Complementary to What?*

The doctrine of complementarity is an interpretation of Heisenberg's uncertainty relations. In the case of mechanical systems, the latter state that it is impossible to know simultaneously, with an arbitrary accuracy, the values of any two conjugate variables, such as the position and the momentum of an electron; in the case of a radiation field, the uncertainty relations consist of similar statements regarding the electric and the magnetic field strengths. The doctrine of complementarity, far from interpreting such mathematical relations in terms of errors of measurements of objectively existent attributes

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¹ L. Rosenfeld, 'Strife about Complementarity' (referred to below as SC), *Science Progress*, July 1953, No. 163, 393, being a revised version of 'L'évidence de la complémentarité', in André George (Ed.), *Louis de Broglie, physicien et penseur*, Paris, 1953

(as is commonly believed), claims that it is meaningless to ascribe simultaneously an objective position and an objective momentum to an electron, or all of its components to a radiation field. Conjugate quantities were called by Bohr *complementary* to each other, in the sense that they are (a) both mutually exclusive, since the sharpening in the value of one of them results in a larger uncertainty regarding the complementary quantity; and (b) both needed to achieve a complete description of experimental results, which the present form of the quantum theory is assumed to yield, at least in the atomic realm.

Owing to the fact that complementary aspects are mutually exclusive, it is impossible—thus Bohr argues—to afford a single well-defined picture of atomic phenomena, being on the other hand indispensable to split the image of reality into two *complementary* models, or pictures, which can be applied in succession, never simultaneously in all rigour, and this simply because the aspects covered by each model are not simultaneously *observed*. In the particular case of entities endowed with mass (such as electrons), one group of variables (position and time) describes the corpuscular aspect, while the group of quantities complementary to these (momentum and energy respectively) describes—as can be seen by recalling de Broglie's relation between momentum and wave-length, and Planck-Einstein's relation between energy and frequency—the wave aspect. In this regard, the contention of the doctrine of complementarity is, that microsystems endowed with mass are neither particles, nor waves, nor wavicles, but that they simply *are* not in themselves, for nothing is supposed to exist apart from the means of observation. Hence, according to complementarity, the words 'particle' and 'wave' designate neither material objects nor properties of material objects; they have no ontological status, but solely an empirical one, for they are only entities entering the description of certain experiments.

Most people believe that the doctrine of complementarity merely expresses the obvious fact that we alter nature whenever we act in order to know it; in other words, that when we perform a measurement we establish an interaction between a piece of apparatus and the object under consideration whereby we unavoidably disturb the latter. This is a valid interpretation of Heisenberg's uncertainty relations, which the doctrine of complementarity attempts to interpret; but that conception is contradictory to the doctrine of complementarity, which is not centred in *things* that are to be observed and that exist

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before and after the acts of observation, but which is centred in *observations*—because, it is argued, it would be ‘metaphysical’ to assume that there is something beyond observational data. It is not merely that the doctrine of complementarity stresses the doubtless active rôle of the experimenter, the active side of knowledge ; it goes beyond this, asserting that observations are the alpha and the omega of knowledge, that there is nothing which is being observed, nothing beyond observation itself.

Bohr has carefully and untiringly explained, for almost a quarter of a century, that we cannot attribute an autonomous physical reality (i.e. a reality independent of the experimenter) to objects at the atomic scale.¹ Philipp Frank, an authorised spokesman of the same philosophical trend, has elucidated this point with his usual clarity, explaining that what we call electron is not a bit of matter but a set of symbols : ‘The “electron” is a set of physical quantities which we introduce to state a system of principles from which we can logically derive the pointer readings on the instruments of measurements.’² Of course, the same is deemed to be valid for the qualities of things ; thus for instance the momentum of an electron ‘has never existed except in so far as we have a set-up which allows the definition of a “momentum”’.³ Things and qualities of things are said to exist only in so far as features of experimental set-ups and acts of observations in themselves.

Now that we are clear about the operational meaning of the concept of reality, we are in a position to understand what is complementary to what. According to Bohr⁴ two things, experimental set-ups, and their corresponding descriptions can all be complementary. When we have an experimental set-up for determining (‘defining’, in the positivistic jargon) one attribute, we destroy the possibility of setting

¹ N. Bohr, *La théorie atomique et la description des phénomènes* (referred to below as *TA*), transl. by A. Legros and L. Rosenfeld, Paris, Gauthier-Villars, 1932, p. 51 ; ‘Licht und Leben’ (referred to below as *LL*), *Die Naturwissenschaften*, 1933, 21, 245-50, p. 247 (see also ‘Light and Life’, *Nature*, 1933, 131, 422, 457) ; ‘Kausalität und Komplementarität’ (referred to below as *KK*), *Erkenntnis*, 1936, 6, 293-303, p. 295 ; ‘Le problème causal en physique atomique’ (referred to below as *PCPA*), in the collective volume *Les nouvelles théories de la physique*, Paris, 1939, 11-32, p. 25 ; ‘Newton’s principles and modern atomic mechanics’ (referred to below as *NP*), in the collective volume ed. by the Royal Society, *Newton Tercentenary Celebrations*, Cambridge, 1947, 56-61, p. 59.

² Philipp Frank, *Foundations of Physics* (referred to below as *FP*), in *International Encyclopedia of Unified Science*, 1, No. 7, Chicago, 1946, p. 54

³ Frank, *FP*, p. 55

⁴ Bohr, *KK*

up the 'complementary' arrangement which would allow us to determine its 'conjugate' attribute.

Notice once again that it is not the numerical value of the attribute that is changed by the act of its measurement—since this would entail that it had a value before its measurement. In all this we have neither atomic objects nor their attributes considered as things-in-themselves: complementarists avowedly do not make statements about the real world, they maintain that quantum mechanics does not speak of real objects that are observed but only of experimental arrangements.¹

On this purely epistemological ground, complementarists have criticised two very common notions. According to one of these, 'Heisenberg's relations say that it is impossible to measure simultaneously the position and the velocity of an electron'. This is wrong, explains Bohr, because it implies that position and velocity are well-defined attributes of the object, whereas the point is just that we are forced to give up the notion of 'autonomous attributes of the object' (*selbständige Attribute des Objektes*).² The second popular notion criticised by the complementarists is that 'The electron has no simultaneously determined velocity and position, these being actually indeterminate'. This interpretation is wrong, says Frank, because it assumes that there is something (the electron with indeterminate properties) that pertains to the real world.³

What is at stake in all this is not the structure of micro-objects, but the whole theory of knowledge with its old struggle between materialism and immaterialism: complementarity is not a physical but a philosophical doctrine, because it does not refer to matter in motion but to concepts and their verbalisations. As Frank says so amusingly, 'All the confusion is produced by speaking of an object instead of the way in which some words are used'.⁴ This fact, that the doctrine of complementarity is of a philosophical, not of a scientific nature, is not willingly accepted by most complementarist physicists who, like Rosenfeld, regard it as 'the most direct expression of a fact'.⁵ But what positivist physicists fail to see is granted by positivist philosophers. Thus Reichenbach in one of his last books wrote:

¹ Philipp Frank, 'Philosophische Deutungen und Missdeutungen der Quantentheorie' (referred to below as *PDM*), *Erkenntnis*, 1936, 6, 303-317, p. 308. See also *Interpretations and Misinterpretations of Modern Physics*, Paris, 1938

² Bohr, *KK*, p. 297

³ Frank, *PDM*, p. 308

⁴ Frank, *FP*, p. 55

⁵ Rosenfeld, *SC*, p. 396

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The duality of interpretations thus assumed its final form : the *and* of de Broglie's discovery does not have the direct meaning that both waves and corpuscles exist at the same time, but has the indirect meaning that the same physical reality admits of two possible interpretations, each of which is as true as the other, although the two cannot be combined into one picture. The logician would say : the *and* is not in the language of physics, but in the *metalanguage*, that is, in a language which speaks about the language of physics. Or, in another terminology, the *and* belongs, not in physics, but in the philosophy of physics ; it does not refer to physical objects, but to possible descriptions of physical objects, and thus falls into the realm of the philosopher.¹

The philosophical nature of all this debate will become more apparent when going over to its central problem, which is also the central problem of philosophy, viz. the question of the relation of subject and object.

2 *Esse est Percipi*

In order to be classified as an idealist one does not need to speak the whole day long about the spirit, or to maintain that life is a dream ; it is enough to maintain that nothing exists or appears by itself, autonomously, independently from *some* mind. Berkeley explained it long ago in his straightforward way :

The table I write on, I say, exists, that is, I see and feel it ; and if I were out of my study I should say it existed, meaning thereby that if I was in my study I might perceive it, or that some other spirit actually does perceive it ; [but] as to what is said of the absolute existence of unthinking things without any relation to their being perceived, that seems perfectly unintelligible. Their *esse* is *percipi* [their being is to be perceived], nor is it possible they should have any existence, out of the minds or thinking things which perceive them.²

Nowadays it is hard to maintain such a subjective idealism in ordinary life ; it is easier to maintain it for a domain accessible only to the specialist—for instance, atomic physics. Thus, we often find the amusing spectacle that subjective idealism is asserted with regard to microscopic events, whereas some sort of materialism is retained

¹ Hans Reichenbach, *The Rise of Scientific Philosophy*, Berkeley and Los Angeles, 1951, pp. 175-176

² Berkeley, *A Treatise concerning the Principles of Human Knowledge*, § 3

for the macroscopic level. The following is an example of this epistemological dualism :

In classical physics it is possible to establish a sharp distinction between the system investigated and the means of observation, and therefore to ignore the latter in framing our conception of the phenomenon. The existence of the quantum of action makes such a distinction impossible because it imposes a limit upon the analysis of the interaction between the system and the apparatus which fixes the circumstances in which we observe it. It is therefore the indivisible whole formed by the system and the instruments of observation which now defines the 'phenomenon'.¹

Bohr has sometimes adopted consistently the idealist point of view, extending it to the macroscopic level. He has argued that, since every observation entails a finite interaction with the instrument, 'one cannot attribute to the phenomena nor to the instrument of observation an autonomous physical reality in the ordinary sense of the word'.² He went so far as to approve Heisenberg's remark that 'ordinary (i.e. macroscopic) phenomena are in a way engendered by repeated observations'.³ But usually he attributes validity to idealism at the atomic level only, a favourite statement being that in the analysis of quantum effects we are faced with the impossibility 'of drawing any sharp separation between an independent behaviour of atomic objects and their interaction with the measuring instruments which serve to define the conditions under which the phenomena occur'.⁴ The central point is thus the negation of the *autonomous* existence of atomic objects.⁵ Since atomism, that stronghold of traditional materialism, could no longer be rejected (as it was in Mach's days), it became advisable to denaturalise it: atoms are at last granted a right to existence, but only on the ideal plane, only as 'auxiliary concepts'.⁶

Once materialism has been disposed of, it is easy to dispense with the notion that everything comes from something else, that is, with causality. Bohr explained clearly, for once, that the rejection of causality was only a *consequence* of the rejection of materialism:

¹ Rosenfeld, SC, p. 395

² Bohr, TA, p. 51

³ Bohr, TA, p. 64

⁴ Bohr, 'Discussion with Einstein on epistemological problems in atomic physics' (referred to below as DE), in P. A. Schilpp (Ed.), *Albert Einstein: Philosopher-Scientist*, Evanston, Ill., 1949, p. 218. See also NP, p. 59

⁵ Bohr, LL, p. 247 and KK, pp. 294-296

⁶ cf. Mario Bunge, 'Mach y la teoría atómica', *Boletín del Químico Peruano*, 1951, No. 16, 12-16

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We have been forced to forego the ideal of causality in atomic physics solely because, as a consequence of the unavoidable interaction between the object of experiment and the measurement instruments—an interaction which cannot be corrected for if these instruments are to allow the unambiguous application of the concepts that are needed for the description of the experiments—we cannot speak any longer of an autonomous behaviour of the physical object.¹

Thus, we see clearly that the celebrated crisis of causality is nothing but a consequence of the adoption of an idealist theory of knowledge : it is not a simple result of modern physics, it is a tenet of modern positivism.

3 *Sozein ta Phainomena*

The most important point in this controversy is that most scientists, at least when they are doing research, share the materialistic principle of the objective existence of a gradually knowable thing-in-itself, whereas positivism maintains that there is no such 'hidden' reality behind appearance, since the object is exhausted by its perception (nowadays by its measurement). This positivistic axiom is very old, but in modern times it was first clearly stated by Berkeley,² who maintained that everything is such as it appears to be, there being no such contrast between appearance and reality, for everything is appearance. Hence the methodological prescription : *sozein ta phainomena, salvare apparentias*, to give account of phenomena (appearances).³

This phenomenalist attitude, typical of positivism since Comte, has been adopted by the upholders of the official philosophy of quantum mechanics, one of whose best representatives, Heisenberg, has explicitly stated that the quantum theory does not assume the existence of a *Ding an sich* behind the phenomena (or appearances).⁴ In a more technical language this is expressed in the principle of observables, according to which physics, or at any rate atomic physics, is only concerned with observable properties—meaning the actually observed ones, with exclusion of all sorts of 'hidden parameters'. Thus,

¹ Bohr, KK, p. 298

² Berkeley, op. cit., §§ 87, 88

³ For the early history of this rule, see Pierre Duhem, *ΣΩΖΕΙΝ ΤΑ ΦΑΙΝΟΜΕΝΑ*, *Essai sur la notion de théorie physique de Platon à Galilée*, Paris, Hermann, 1908

⁴ Werner Heisenberg, *Wandlungen in den Grundlagen der Naturwissenschaften*, 7th ed., Zürich, 1947, p. 86

physics is not presented as the investigation of what Bacon called *natura libera* (such as it is without our intervention) through the *natura vexata* (such as it becomes when we subject it to our cognitive actions)—but as the examination of appearances, the latter being conceived (as we shall see) as unanalysable wholes.

For instance, Bohr¹ warns especially against the use of phrases such as 'disturbance of phenomena by observation', i.e. against the use of the concept of vexed nature. The reason is plain: such phrases imply the assertion of the objective existence of a reality hidden, for the time being, behind the appearances; of a *natura libera* existing while we are not acting upon it. That is why Bohr redefined the notion of phenomenon so as to eliminate from it every reference to observed things; in fact, he repeatedly advocated the 'limitation of the use of the word *phenomenon* to refer exclusively to observations obtained under specific circumstances, including an account of the whole experiment'.² All this is admitted and elucidated by Weizsäcker, who suggests that one should not condemn causality as such but only the notion of *objective* causality and, in general, the notion of thing in itself in the sense of existing independently from the subject.³ Being a learned theologian, he does not conclude from this the validity of materialism, as Rosenfeld does, but he confirms his mystic faith. He would never dream of saying that 'From the dialectical point of view it is almost self-evident to observe that the essential part played by the observer in the definition of the phenomena is perfectly consonant with the fundamentally materialistic character of science'.⁴

We have already observed the inconsistency of maintaining subjective idealism for the atomic realm and materialism for the macroscopic level (sec. 2). Atoms do not exist apart from instruments, maintain idealists. Now, the instruments have avowedly an atomic structure—so far not taken into account by the theory. So that, if one asserts that 'Only instruments exist', then one is implicitly stating the proposition that is contradictory to that, namely, 'Atoms exist objectively as well'. This is why Bohr always insists that instruments must be treated in a classical (i.e. macroscopic) way—just in order to

¹ Bohr, *DE*, p. 237

² N. Bohr, 'On the Notions of Complementarity and Causality', *Dialectica*, 1948, 2, 312-319, p. 317. See also *PCPA*, p. 24 and *DE*, pp. 237-238

³ Carl Friedrich von Weizsäcker, *Zum Weltbild der Physik*, 5th ed., Stuttgart, 1951, pp. 30, 41-42, 76 and *passim*

⁴ Rosenfeld, *SC*, p. 407

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prevent a further analysis of the famous interaction, an analysis that would show what the contribution of each, object and subject, is to the phenomenon. Pauli avoids the mentioned inconsistency by taking a further step ; he declares that not only must we deal with observations without implying observed things, but we must not even deal with *actual* observations—only with possible ones : ‘The actual observation appears as an event outside the range of a description by physical laws and brings forth, in general, a discontinuous selection out of the several possibilities foreseen by the statistical laws of the new theory.’¹ Thus, according to the representatives of the official philosophy of quantum mechanics, as for Berkeley, observations must be accepted at their face value and every attempt to analyse and understand them is forbidden forever.

It seemed as though, from now on, we could not be sure whether we are observing the object, or whether the object is observing us, or whether it is observing itself, or whether we are not doing physics but introspective psychology : the obsolete distinction between subject and object is no longer valid, complementarists say ; in its place we have an unanalysable muddle—not precisely a unity in which both terms interact in a determined way, but just a ‘sealed unit’ which we are forbidden to look into. It is consistent to match such a stand with any sort of idealistic philosophy. But it is difficult to understand why Rosenfeld should advocate, in the name of materialism, the rejection of the distinction established by ‘the narrow and antiquated brand of materialism’ between subject and object.²

4 *Not a Departure from Objectivity ?*

Is this not a departure from the ideal of objectivity ? Weizsäcker³ admits it openly and with joy ; Rosenfeld denies it.⁴ He argues on an analogy with the theory of relativity, whose objective content he sees in the invariance of the form of its equations, with respect to certain groups of transformations, or with respect to the choice of the mode of reference. (In this connection it is interesting to remark that Bohr,⁵ on the other hand, thinks that relativity is just the recognition of the essential dependence of every physical phenomenon upon the observer.)

¹ Wolfgang Pauli, *Exclusion Principle and Quantum Mechanics*, Nobel Prize lecture, Neuchâtel, ed. du Griffon, 1947, p. 18

² Rosenfeld, SC, p. 405

⁴ Rosenfeld, SC, p. 405

³ Weizsäcker, op. cit.

⁵ Bohr, LL, p. 247, and KK, p. 294

It seems to the author that Rosenfeld wrongly identifies *objective* (pertaining to or related to the object) with *absolute*, which in physics means independent from standards and frames of reference. Such an identification (and the correlative of subjective with relative) was held by Newton, but is now untenable. The Doppler effect is a classical example of an objective though relative phenomenon: objective, because it is produced independently of the existence of human beings, independently of the subject; relative, because it is not the same for all material systems irrespective of their state of motion (it does not involve one object but a potential infinity of objects). The relativistic increase of mass with velocity is often presented as an apparent phenomenon depending on the observer—which is also false. It is certainly relative, but it is also objective, because it takes place independently of its being observed or not; and, thanks to the convertibility of kinetic energy into radiation energy, by accelerating an electron in the betatron we are rewarded with X-rays that are as objective as the causes producing them. Thus not only invariant but also relative entities may be perfectly objective. On the other hand, absolute entities and relations may not have an objective meaning.

The theories of relativity work with objective facts—some of them relative and others absolute; and, in giving account of reality, they make use of ideal entities, which in turn may be relative or absolute. Relativity, like any other branch of physical science, is concerned only with objective facts, never with facts essentially dependent upon the observer (i.e. subjective facts)—in spite of Bohr's statements. Relative entities are expressed in a non-invariant way, whereas absolute entities are expressed in an invariant way—with the proviso that the categories 'relative' and 'absolute' are in turn relative, for they refer always to a certain set of transformations; whereas, on the other hand, the degree of objectivity of a theory is not dependent on the extent of its invariance, but on the extent of its agreement with the objects to which it refers. The observer, who plays such a central rôle in the positivistic presentations of relativity, is, as in the case of quantum mechanics, just *one* of the possible material systems entering into a relative phenomenon.

Contrary to Rosenfeld, I think that the character of objectivity of a set of symbols does not depend upon their properties of invariance (which, let us repeat, is relative to a given set of transformations), but only upon the *physical meaning* attached to them. That is, it does not depend upon the form of the equations but upon their *content*—upon what logical empiricists call the semantical rules. If, within the

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context of a given theory, we say that 'The symbol x stands for the position of a mass-point', this statement will form part of the objective content of that theory in so far as mass-points can be said to exist, and even if we are not able to measure the effective value of x . This statement will have an absolute meaning in so far as quality is concerned (since x will not cease to represent a position, it will not turn into a momentum, for example); and it will have a relative meaning as regards quantity, since the numerical value of x will depend on the system of reference (whether or not an actual measurement is performed). In a simple definition such as that we can understand how objectivity is not necessarily linked with invariance or absoluteness.

Mutatis mutandis, the same applies to quantum theory. Rosenfeld asserts that the objective content of this theory, 'the objective expression for the quantal laws of nature', is represented by the equations connecting the operators among each other, because these equations (for example, the commutation rules) are invariant under the canonical transformations, 'which express the passage from one mode of observation to another'.¹ Now, the operator equations are susceptible to an infinity of representations which, when *explicit* reference to observational data is made, refer each to a 'mode of observation', to a 'particular condition of observation'. The first point I wish to stress is that such statements amount to the assertion that the quantum laws *do* refer to objects existing independently from our acts of observation; they imply that there is a *unique* reality *behind* the countless appearances, behind our representations of that reality. And this plainly contradicts Rosenfeld's basic contention that *no* such separation of the object from the subject is even conceivable. So that, without noticing it, he is telling us that we may retain scientific objectivity on condition that we do *not* accept the epistemological foundations of the usual interpretation of quantum theory—which is a fine piece of empiricist logic.

But there is more to it: once again Rosenfeld confuses, I believe, 'absolute' and 'objective'. A choice of representation, contrary to his remark, does not necessarily involve subjectivity—the same as the choice of a frame of reference does not eliminate objectivity; it only involves, from the mathematical point of view, a specialisation. The canonical invariance of certain basic equations do not provide the objective content of a theory. One might construct, and this is

¹ Rosenfeld, *SC*, p. 406

actually being done every day, theories that are invariant under a host of transformations, but which simply do not work, that is, which have no objective content—as happens with most meson field theories. On the other hand, we may limit ourselves to the choice of a special representation (in particular, to the choice of space and time as basic variables) and still obtain most if not all of the verified results of quantum mechanics. Moreover, what will happen to the mathematically beautiful framework of transformations in Hilbert space the day that the present basic equations (such as the wave equations) are recognised as just linear approximations to non-linear equations? As has been pointed out by Bohm,¹ if we accept such a possibility, the whole framework breaks down and we are compelled to sacrifice *mathematical* generality and choose, for the benefit of *physical* generality, a special representation. And this is not idle speculation, for we know from experience in other fields of physics that linearity is not an absolute and ultimate quality of nature but only an approximation of our knowledge of it.

To sum up, Rosenfeld seems to be inconsistent when he identifies objectivity with absoluteness, or invariance, for he is then led to admit implicitly that matter exists independently of its being perceived—which runs counter to the official philosophy of quantum mechanics. Further, I think he is wrong in asserting that identity, because there are relative objectivities as well as absolute subjectivities.

¹ David Bohm, 'A Suggested Interpretation of the Quantum Theory in Terms of "Hidden Variables"' (referred to below as *IQT*), *Physical Review*, 1952, **85**, 166, 180. See also 'Comments on an Article of Takabayasi concerning the Formulation of Quantum Mechanics with Classical Pictures', *Progress of Theoretical Physics*, 1953, **9**, 273.

(To be concluded)

WHY PHYSICAL SPACE HAS THREE DIMENSIONS *

G. J. WHITROW

And the first step of the *Peripatetick* argument is that, where *Aristotle* proveth the integrity and perfection of the World, telling us, that it is not a simple line, nor a bare superficies, but a body adorned with Longitude, Latitude and Profundity ; and because there are no more dimensions but these three ; the World having them, hath all, and having all, is to be concluded perfect. And again, that by simple length, that magnitude is constituted, which is called a line, to which adding breadth, there is formed a Superficies, and yet further adding the altitude or profundity, there results the Body, and after these three dimensions there is no passing farther, so that in these three the integrity, and to so speak, totality is terminated, which I might but with justice have required *Aristotle* to have proved to me by necessary consequences, the rather in regard he was able to do it very plainly and speedily.

GALILEO's first dialogue on the *Two Principal Systems of the World* opens with this query raised by Salviatus¹ concerning the three-dimensional nature of the physical universe. Although the dialogue was ostensibly a debate on the rival merits of the Copernican and Ptolemaic world-systems, Galileo was attacking the philosophy and cosmology of Aristotle and the Aristotelians. Aristotle's *De Caelo* begins with a discussion of the dimensions of spatial objects and of the world, and it was no coincidence that Galileo's polemic is first directed to the same subject.

Like so many other problems of cosmology that of the dimensionality of the universe continues to baffle us. We now realise that the problem is a dual one, although in the past this was not always made clear. First, there is the question of what is *meant* by saying that the universe appears to have a certain number of spatial dimensions. This is a mathematical problem. Then, there is the more elusive problem of explaining why this number is precisely *three*. In surveying the history of the latter problem we find that four alternative

* Received 9. iii. 54

¹ T. Salusbury, *Mathematical Collections and Translations*, I, London, 1661, pp. 2 sq.

possibilities have been advocated : our attribution of three dimensions to physical space has been regarded as (i) purely contingent, (ii) a necessary feature of our conception of the world, (iii) partly conventional and partly contingent, or (iv) partly necessary and partly contingent, so that it can be accounted for as the essential concomitant of some 'deeper' contingent feature of our conception of physical existence.

In the present paper I shall confine attention to possibilities (ii), (iii) and (iv). In recent times Eddington has advocated (ii) and Poincaré (iii). The ultimate aim of this paper is to advance a new argument in favour of (iv), but before presenting this argument I shall make a rapid survey of the history of the subject. This will embrace not only its *mathematical* aspect but also the study of our *perception* of three dimensions in visual, tactile and motor space. Such a survey not only places the new suggestion in proper perspective, but also reveals that any new suggestion may prove to be a significant advance, since, with one notable exception, so little progress has been made in the past twenty-five centuries towards explaining why it is that space has just three dimensions, no less and no more.

I *The Mathematical Formulation of the Problem*

The ancient Greek geometers realised very early that the dimensionality of physical objects was of crucial importance in their science. They discovered that, whereas there is an *infinite* number of regular polygons in a plane, there are only *five* regular solids. As is well known this result was employed by Plato in his theory of the physical world set forth in the *Timaeus*. (Two thousand years later Kepler appealed to the same theorem in his, to our minds, equally curious *a priori* attempt to show that there could only be six principal planets : Mercury, Venus, Earth, Mars, Jupiter and Saturn.) Plato argued that the most perfect and regular figure in which all these regular solids can be inscribed—the sphere—is the appropriate figure to associate with the universe as a whole. He was greatly influenced by Parmenides in whose cosmology everything appeared to be sacrificed for the sake of logical rigour. Parmenides regarded the 'real universe' as being a complete unity and denied the reality of all plurality. In his *Way of Truth* he argued with remorseless logic that one property after another *cannot* apply to the 'real universe'. Finally, after this long sequence of negations he concluded with the positive

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assertion that it is a sphere. 'But since there is a furthest limit, it is complete on every side, like the mass of a well-rounded Sphere, everywhere equally poised from the midst.'¹ Although it has been pointed out that Parmenides' description of the *ἐν ὅν πᾶν* is 'clogged with all the forms of materiality',² it has not been sufficiently stressed by scholars that Parmenides made a surreptitious appeal to the 'Way of Seeming' in his tacit assumption that Being is spatially three-dimensional.³ Plato himself, it must be admitted, went some way towards pointing this out when in his dialogue *Parmenides* he argued that for consistency Parmenides should have refrained from assigning *any* shape to Being. Nevertheless, he failed to realise that no geometrical dimension could be assigned, and instead contented himself with the argument that since a sphere must have a definite boundary which can be distinguished from the interior, 'the One would be no longer one, but two', an argument which the discovery of non-Euclidean spherical geometry (employed by Einstein in his theory of the finite *unbounded* universe) has completely demolished.

The reason for Parmenides' illicit appeal to tri-dimensionality and for Plato's failure to comment on it is clear. They saw no reason to question the 'completeness' of the concept. As Aristotle wrote in *De Caelo*

A magnitude if divisible one way is a line, if two ways a surface, and if three a body. Beyond these there is no other magnitude, because the three dimensions are all that there are . . . We cannot pass beyond body to a further kind, as we passed from length to surface and from surface to body. For if we could, it would cease to be true that body is complete magnitude. We could pass beyond it only in virtue of a defect in it; and that which is complete cannot be defective, since it has being in every respect.⁴

We see here that Aristotle first attempted to define what he meant by saying that a body has three dimensions, and then went on to deny the possibility, *even in thought*, of a body having more than three dimensions. Nevertheless, Aristotle realised that there is a question to

¹ F. M. Cornford, *Plato and Parmenides*, London, 1939, p. 44

² R. D. Archer-Hind, *The Timaeus of Plato*, London, 1888, p. 8

³ If he had merely wished to say that the limit is everywhere equidistant from the centre he could equally well have likened the universe to a circle.

⁴ W. D. Ross (Ed.), *The Works of Aristotle*, Oxford, 1947, 2, 268a

be asked and he attempted to answer it. Although his attempts at solution strike us as quaint, due credit should be given to him for realising the existence of the problem. Typical was his argument, derived from the Pythagoreans, that since an 'all' must have a beginning, a middle and an end, and these are three, it follows that the number of the world is the triad, which reveals itself in the three spatial dimensions. This solution was criticised with characteristic sarcasm by Galileo (*loc. cit.*) through his mouth-piece Salviatus :

To tell you true, I think not myself bound by all these reasons to grant any more but only this, That that which hath beginning, middle and end, may and ought to be called perfect : But that then, because beginning, middle and end, are Three, the number Three is a perfect number, and hath a faculty of conferring *Perfection* on those things that have the same, I find no inducement to grant ; neither do I understand, nor believe that, for example, of feet the number three is more perfect than four or two, nor do I conceive the number four to be any imperfection to the Elements ; and that they would be more perfect if they were three. Better therefore to have left these subtleties to the *Rhetoricians* and to have proved his intent, by necessary demonstration ; for so it behoves to do in demonstrative sciences.

Although Galileo's own attempt to solve the problem of why there appear to be three spatial dimensions was no more successful than Aristotle's, credit must be given to him for formulating the concept much more clearly. Indeed this is what his demonstration really amounts to. He introduced, through Salviatus, the orthogonal triad of lines AB , AC , and AD , and maintained that 'there cannot concur any more lines in the said point, so as to make therewith right-angles and the dimensions ought to be determined by the sole right lines which make between themselves right-angles ; therefore the dimensions are no more but three'. Simplicius, the fictional spokesman of Aristotle, retorted 'And who saith that I cannot draw other lines ?' He suggested that one might draw one 'underneath, unto the point A , that may be perpendicular to the rest'. In the light of our superior knowledge, it is fascinating to observe here one of the most remarkable 'near-misses' in the history of science. Instead of anticipating by three centuries the invention of higher space, Galileo continues to assert through Salviatus that no more than three mutually orthogonal lines can meet in the same point. Sagredo, the spokesman of the intelligent layman, comments

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I see what *Simplicius* means, namely, that should the said *DA* be prolonged downwards, then by that means there might be drawn two others, but they would be the same with the first three, differing only in this, that whereas now they only touch, then they would intersect, but not produce new dimensions.

It is of great interest to set this passage side by side with a tantalising paragraph from the commentary of the historical *Simplicius* (of *Cicilia*) on Aristotle's *De Caelo* where he refers to the lost work *περὶ διαστάσεως*, of Ptolemy (*circa* A.D. 150) :

The admirable Ptolemy in his book *On Distance* well proved that there are not more than three distances, because of the necessity that distances should be defined, and that the distances defined should be taken along perpendicular lines, and because it is possible to take only three lines that are mutually perpendicular, two by which the plane is defined and a third measuring depth ; so that if there were any other distance after the third it would be entirely without measure and without definition. Thus Aristotle seemed to conclude from induction that there is no transfer into another magnitude, but Ptolemy proved it.¹

Even from this brief account it is clear that Ptolemy failed to produce what we should regard as a satisfactory *proof*; nevertheless, in view of *Simplicius*' apparent realisation of the inadequacy of Aristotle's approach, we cannot but regret the loss of Ptolemy's book.

Although the origins of our problem lie in the work of the great Greek geometers, they were concerned with plane and solid configurations rather than with the concept of space as such, unlike Plato who regarded space as a pre-existing framework without which there could be no visibly extended universe. In the scientific renaissance of the seventeenth century, Newton, like Plato, attributed an independent existence to physical space, whereas his rival Leibniz believed only in the independent objective existence of bodies and regarded space merely as an ordering of bodies among themselves. Leibniz argued that if space were something in itself it would be impossible to give a reason why bodies should be arranged as they are and not in the opposite order by turning East and West about. Kant pursued this point further. He raised the apparent paradox of the coexistence of congruent but not superposable (enantiomorphous) bodies, for

¹ *Simplicius of Cicilia, Simplicii in Aristotelis De Caelo Commentaria*, ed. J. L. Heiberg, Berlin, 1894, 7a, 33

example, the left hand which, despite all equality and similarity cannot be enclosed within the bounds of the right, so that the glove of one hand cannot be used for the other.¹ Neither he nor contemporary geometers realised that this 'paradox' could be resolved by increasing the number of dimensions of space. The same 'paradox' arises in the plane unless we are allowed to rotate congruent figures out of the plane so as to superpose one on another. In four-dimensional space the left glove *can* be fitted on to the right hand.

The invention of the geometries of higher space was one of the supreme triumphs of human imagination, perhaps no less remarkable than the invention of curved space and the non-Euclidean geometries of Lobatchewsky, Bolyai, and others. Indeed, the latter invention was slightly the earlier, for although the Cambridge Platonist Henry More had suggested as long ago as 1671 that spirits have four dimensions,² it was not until 1827 that the first significant contribution to the synthetic geometry of higher space was made, when the geometer Möbius pointed out that two enantiomorphous solids could be brought into coincidence by a four-dimensional rotation. Although the development of algebra and analysis had long before suggested the possible formulation of the concept of higher space, previous geometers had expressly rejected it. Indeed, the quasi-empirical geometrical conception of equations and the geometrical form of their solution had actually hindered the progress of algebra with the ancients. Higher equations than the third were avoided as 'unreal'. This influence persisted with the rise of modern mathematics in the sixteenth and seventeenth centuries. For example, in 1553 Michael Stifel,³ in his revision of Rudolph's *Coss* (algebra), wrote of 'going beyond the cube just as if there were more than three dimensions', remarking, 'which is against nature'. Again, in 1685, John Wallis objected to the 'ungeometrical' names given to the higher powers (for example, the term *sursolide* occurs several times in the geometry of Descartes) and he called one of them, a four-dimensional 'plano-plane', a 'Monster in Nature, less possible than a Chimera or Centaure!'. In his view 'Length, Breadth and Thickness take up the whole of Space'; and he even made the sweeping assertion, 'Nor can Fancie

¹ E. Belfort Bax (Trans.) *Kant's Prolegomena*, London, 1883, p. 33

² H. More, *Enchiridion Metaphysicum*, London, 1671, Pt. 1, ch. 28, p. 384

³ Born 1487, died 1567, he was 'a priest, a reformer and a fanatic, but was one of the most skilful arithmeticians of his time'. (D. E. Smith, *Rara Arithmetica*, Boston, 1908.)

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imagine how there should be a Fourth Local Dimension beyond these Three'.¹ A century later Kant made some progress towards the modern view when discussing the apodeictic certainty of all geometrical proportions and the *a priori* necessity of space. He argued that if the concept of space were acquired *a posteriori* we should 'only be able to say that, so far as hitherto observed, no space has been found which has more than three dimensions'.²

It was not, however, until the middle of the nineteenth century, and after the creation of non-Euclidean geometry, that the possibility of geometry in more than three dimensions was systematically developed, algebraically and analytically, by Cayley, Grassmann, Riemann and somewhat more synthetically by Schläfli, whose *Theorie der vielfachen Continuität* was completed, but not published, in 1852. Although it may be true that, as Poincaré once wrote, *Un homme qui y consacrerait son existence arriverait peut-être à se peindre la quatrième dimension*, to this day only one or two people have ever attained the ability to visualise hyper-solids as simply and naturally as ordinary solids.³

For Galileo, as apparently for Ptolemy, physical space was three-dimensional because at any point a triad of mutually orthogonal straight lines can be constructed. This idea was exploited by Descartes in his development of algebraic geometry and ultimately led to the more profound idea that any point of an n -dimensional space can be identified by n independent parameters not necessarily associated with orthogonal sets of straight lines. Nevertheless, by the beginning of the present century two remarkable mathematical discoveries made urgent a still deeper investigation of the precise nature of the dimensional concept. In establishing a one-one correspondence between the points of a line and the points of a plane, Cantor exploded the intuitive assumption that a plane must contain more points than a line; and, likewise, by his discovery of the continuous mapping of a linear interval on the whole of a square, Peano exploded the belief that the dimension of a space could be defined as the least number of continuous parameters required to describe it. Poincaré, who declared in 1912 that 'Of all the theorems of analysis situs (topology), the most important is that which we

¹ J. Wallis, *A Treatise of Algebra, Both Historical and Practical*, London, 1685, p. 126

² I. Kant, *Critique of Pure Reason* (tran. N. Kemp Smith), London, 1934, p. 44

³ H. S. M. Coxeter, *Regular Polytopes*, London, 1948, p. 119

express by saying that (physical) space has three dimensions',¹ attempted to formulate a recursive definition, namely, that a space is n -dimensional whenever its walls are $(n - 1)$ -dimensional, the null-set having the conventional definition $- 1$. Brouwer produced a *Gegenbeispiel* in the double-sheeted cone, arguing that Poincaré's definition would assign the 'wrong' dimension 1 and not the 'correct', i.e. the usually accepted, dimension 2 to it. In 1911, Brouwer conclusively proved that, despite Cantor's and Peano's results, there is a genuine distinction between spaces of different dimensions. He showed that it is not possible to establish a correspondence which is *both* one-one and continuous between a Euclidean m -space and an n -space. Such spaces are not *homeomorphic* unless $m = n$.² It was not until 1922, however, that a completely satisfactory definition of the dimensional concept was finally formulated by Menger and Urysohn. In Menger's formulation³ it reads :

- (i) the empty set has the conventional dimension $- 1$;
- (ii) the dimension of a space is the least integer n for which every point has arbitrarily small neighbourhoods whose boundaries have dimensions less than n .

These abstract mathematical developments have a direct bearing on our problem of the dimensionality of physical space. They not only enable us to define the concept clearly but confirm our intuitive conception of a fundamental (topological) distinction between spaces of different dimensions. Moreover, the group structure of the Euclidean group of rotations is different for the various numbers of dimensions. In consequence, Weyl has been led to express the pious hope that 'mathematical and physical laws may cease to be indifferent to the number of dimensions on some deeper level than has been touched by the physics of today'.⁴ In this way he suggests that the inner peculiarities of three-dimensional space may lead to a 'reasonable' explanation of the fact that God, in creating the world, chose just this number of spatial dimensions and no other. Although this suggestion

¹ H. Poincaré, *The Value of Science* (trans. G. B. Halsted in *The Foundations of Science*), New York, 1929, p. 240

² L. E. J. Brouwer, *Math. Ann.*, 1911, 70, 161-165 ; *Journal f. Math.*, 1913, 142, 146-152

³ K. Menger, *Dimensionstheorie*, Leipzig, 1928

⁴ H. Weyl, *Philosophy of Mathematics and Natural Science*, Princeton, 1949, p. 136

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remains at present no more than a hopeful possibility for the future, it may well prove to be penetrating.¹

From this brief survey of the labours of geometers and mathematical philosophers we see that their main contribution to our problem has been the basic one of enabling us to *formulate* it clearly and meaningfully. When we come to survey past attempts to *solve* the problem we must take into account the processes responsible for our *perception* of three-dimensional space.

2 *The Origin of our Awareness of Three Spatial Dimensions*

The problem of how we actually perceive that space has three dimensions has a long history, dominated by the fact that at least three important kinds of perceptual space have come to be distinguished—visual, tactile, and kinaesthetic or motor. Vision depends on the retinal image formed within the eye, but this image is formed on a two-dimensional surface. Nevertheless, we see the world as extending to the horizon. How is this possible? To many investigators since the seventeenth century the visual sense alone has seemed inadequate to account for our knowledge of three-dimensional space (and indeed, to some, for our belief in an ‘external’ world). The classic exponent of this view was Berkeley. In his *Essay towards a New Theory of Vision* which was published in 1709, he maintained that distance of itself cannot be seen, and that the retinal image is inadequate for this purpose.² ‘For *distance* being a line directed end-wise to the eye, it projects only one point in the fund of the eye, which point remains invariably the same, whether the distance be longer or shorter.’³

In his *Essay* Berkeley drew attention *inter alia* to the effect of the convergence and accommodation mechanism of the eye on our visual perception of distance. In 1833 Wheatstone invented the stereoscope, and thereby demonstrated experimentally the relevance of binocular vision to this perception. To the present day there has been controversy concerning the relative contributions of these two

¹ Cf. the history of modern crystallography since the classical researches on molecular asymmetry by Pasteur.

² As was pointed out by Norman Smith (*British Journal of Psychology*, I, 1905, 191-204), although this has been spoken of as Berkeley’s great discovery it was recognised by all contemporary writers on optics and in particular underlies Malebranche’s theory of the perception of distance and magnitude (*Recherche de la vérité*, liv. I, ch. ix, Paris, 1st ed. 1674).

³ G. Berkeley, *A New Theory of Vision*, § 2

mechanisms to three-dimensional vision. A profound study of the problem was made by Helmholtz in his classic *Treatise on Physiological Optics*.¹ He laid particular stress on the difference between our perceptions of direction and of distance. For example, looking at the starry sky, we find objects of whose form, dimension, and distance we have no previous idea at all. In this case, no advantage can be derived either from using both eyes or from any movements which we can make. In these circumstances objects which are extended in space of three dimensions appear to us as having only two—indeed, until comparatively recent times, the stars were thought to be all at the same distance from us. Helmholtz was a protagonist of the theory that visual space-perception involves some form of mental synthesis, as for example in the correlation of the sense of vision, the sense of touch and the movements of the body associated with grasping for the seen object.

Recently, however, it has been shown (I think fairly conclusively) that the retinal image plays a far more important part in the visual perception of distance than had previously been supposed. According to the classical theories of space perception the third dimension is regarded as a line directed outwards from the eye into an empty space between the observer and the object seen. In this book, *The Perception of the Visual World*, J. J. Gibson points out that this is much too simple a way of looking at things; instead, one must fix one's eyes on some prominent point and then pay attention not to that point alone, but to the whole range of what you can see, keeping one's eyes still fixed. The attitude to take is that of the perspective draughtsman. It may help if you close one eye.² The so-called *visual field* thus revealed has a gradient which Gibson calls the 'gradient of the density of texture'. It is a gradient from coarse (near) to fine (distant), which is beautifully illustrated in his book by a photograph of a closely packed group of barrels which gives an immediate impression of depth. Gibson argues that this gradient of density of texture is an adequate stimulus for the impression of continuous distance and is the most important retinal correlate of visual depth.

Turning now to the consideration of alternative ways in which space can be perceived, we note that the great importance of Berkeley's

¹ H. L. F. von Helmholtz, *Treatise on Physiological Optics* (Ed. J. P. Southall), 3, Optical Soc. America, 1925, pp. 158 sqq.

² J. J. Gibson, *The Perception of the Visual World*, Cambridge (Mass.), 1950

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work is due to the fact that, unlike his forerunner Malebranche, he directed attention to the important part which *touch* plays in the visual apprehension of distance and magnitude. In his *Essay* he appealed to our sense of touch to account for our awareness of the third dimension but assumed that tactile space needs no explanation.

A different approach to the problem of the perception of three-dimensional space was made by Ernst Mach. In his essay *Space and Geometry in the light of Physiological, Psychological and Physical Inquiry*, he drew attention to the significance of animal movement for the evolution of the idea of space. Untrammelled orientation and the interchangeability of every orientation with every other (although orientation in the direction of gravity is not purely optional) were, he argued, the ultimate origins of the idea of space as isotropic or equal in all directions. Progressive motion and the possibility of orientation in any direction together rendered space infinite and homogeneous. The three-dimensional character attributed to space was due to the fact that man in common with many animals has three cardinal directions marked on his body.

Above and below, the bodies of such animals are unlike, as they are also in front and behind and to the right and to the left. To the right and left, these animals are apparently alike, but their geometrical and mechanical symmetry, which subserves purposes of rapid locomotion, should not deceive us with regard to their anatomical and physiological asymmetry. Though the latter may appear slight, it is yet distinctly marked in the fact that species very closely allied to symmetrical animals sometimes assume strikingly unsymmetrical forms. The asymmetry of the plaice (flatfish) is a familiar instance, while the externally symmetric form of the slug forms an instructive contrast to the unsymmetric shapes of some of its nearest relatives. This trinity of conspicuously marked cardinal directions might indeed be regarded as the physiological basis for our familiarity with the three dimensions of geometric space.¹

Mach's discussion thus focused attention on the parts played by the structure and movements of our bodies in the development of our idea of physical space. This way of studying our problem was continued with great acuteness and originality by Poincaré. He argued that visual space was more artificial than motor space. Spectacles of suitable design could destroy the accord between the

¹ E. Mach, *Space and Geometry in the light of Physiological, Psychological and Physical Inquiry* (trans. T. J. McCormack), Chicago, 1906, p. 18

feelings of convergence and accommodation of the eyes. Are we then to say that the optician could thereby give one more dimension to space? Poincaré rejected this possibility and considered instead the dimensions of motor space. He drew attention to the part played by the semicircular canals, which experiments had recently shown were necessary for our sense of orientation. Nevertheless, he rejected the suggestion that the three-dimensional nature of space is a consequence of the fact that we have just three pairs of these canals.

The three pairs of canals would have as sole function to tell us that space has three dimensions. Japanese mice have only two pairs of canals; they believe, it would seem, that space has only two dimensions, and they manifest this opinion in the strangest way; they put themselves in a circle, and, so ordered, they spin rapidly around. The lampreys, having only one pair of canals, believe that space has only one dimension, but there manifestations are less turbulent.¹

This picturesque theory, he maintained, was inadmissible because the nerves of the canals can inform us only of the difference of pressure on the two extremities of the same canal and hence of the rotations of the head but not of the translations it may undergo nor of the relative movements of the other parts of the body in regard to the head or to one another.

Instead, Poincaré adopted a solution midway between that of the pure Kantians and the pure empiricists. Kant regarded the idea of space as a pure intuition prior to all experience. Mach argued that if our sensations of space were independent of the quality of the stimuli which go to produce them, then we might make predications concerning the former independently of external or physical experience. 'But this basis is unquestionably inadequate to the complete development of a geometry, inasmuch as concepts, and in addition thereto concepts derived from experience, are also requisite to this purpose.'² Poincaré believed that the mind constructs space, but not out of nothing. The materials and models required pre-exist within the mind, but there is not a single model imposed upon it. The mind has choice, and in principle can choose between space of four and space of three dimensions. 'What then is the role of experience? It gives the indications following which the choice is made.'³ Thus Poincaré concluded that the dimensional nature of space is *partly conventional*

¹ H. Poincaré, *op. cit.* p. 277

² E. Mach, *op. cit.* p. 34

³ H. Poincaré, *op. cit.* p. 276

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but not wholly so, since experience has taught us that it is more convenient to assign three rather than any other number.

Recent developments in the experimental and observational investigation of the behaviour of animals in relation to their spatial environment promise to throw fresh light on these theories. A bird like the bustard, which normally inhabits great open plains, cannot lift its legs automatically over a low obstacle, but will go on knocking against it until finally it makes a rush and tumbles across. Its world is effectively two-dimensional. On the other hand, a tree-living bird is automatically provided with a nervous mechanism which enables it to fit spatial relations into a three-dimensional framework. It has been suggested that Kant's intuition of space is the mental correlate of such a mechanism in our own nervous system. Experiments with chimpanzees indicate that spatial relations and configurations are much more effective in determining their actions than shapes or colours.¹ Our 'intuition' of space, i.e. the recognition of the spatial order of objects, in particular its three-dimensional nature, would thus appear to be (i) a very primitive property impressed by the external world on the mind, (ii) to be itself 'shaped' by the mind and nervous mechanism, and then (iii) to become a means whereby the world is visualised.

3 Possible Solutions of the Problems

We have seen that prior to the mathematical discovery of higher space, the view was held by many natural philosophers that its three-dimensional character was a necessary attribute of physical space. Following the discovery of higher space and also as a consequence of prolonged investigation of our perception of physical space, Poincaré was led to argue that our awareness of the three spatial dimensions is *partly conventional and partly contingent in origin*. Despite these developments, no real advance has yet been made towards explaining why we find or attribute just three dimensions and not any other number, except for a remarkable attempt due to Eddington.

(a) *Eddington's Solution*. Eddington claimed that the three-dimensional character of space was the physical correlate of a property of *E*-frames in his wave-tensor calculus.² The root idea

¹ G. Burniston Brown, *Science: its Method and Philosophy*, London, 1950, ch. 1

² A. S. Eddington, *Fundamental Theory*, Cambridge, 1946, p. 124

is discussed more simply in his *Turner Lectures*.¹ Ignoring all reference to the structure of our bodies and our spatial perception, Eddington appealed to a theory of measurement. He argued that any measurement involves in principle a comparison of two entities each having two limits, e.g. two rods, one being our adopted standard. Since there are two ends to each rod a measurement involves four entities. By a difficult and complicated argument he claimed that this leads to a four-dimensional space-time which he was able to separate into three-dimensional space and one-dimensional time. Eddington's argument, despite its originality and ingenuity, cannot be regarded as a strict logical proof. On page 124 of *Fundamental Theory* we read : 'To what extent this amounts to an *a priori* proof that the space-time of physical experience must be of this kind, depends on our inquiry into the ultimate origin of the *E*-frame in Chapter XIII.' When we turn to Chapter XIII we find that it appears as an Appendix which, had the author lived, would have been expanded. This appendix is a reprint of a paper on 'The Evaluation of the Cosmical Number' in which Eddington seeks to determine this number directly from the principles of measurement. In the third paragraph, however, we read :

A logically complete demonstration, if it is possible, would be extremely prolix ; and it is not the kind of problem I could myself attempt. But I shall try to show that at each stage the investigation is being driven by its own momentum—that the moves leading to a universe of *N* particles are forced. Or at least there is so much pressure behind the moves that, when we find the physicist actually does think there are *N* particles, there can be no doubt that it is the result of this pressure and not because of any peculiarity in the external world.²

Whatever the ultimate verdict on Eddington's theory may be I do not believe that it will lead to the general acceptance of the view that

¹ A. S. Eddington, *The Philosophy of Physical Science*, Cambridge, 1939, p. 169

² I believe that a good deal of criticism of Eddington, particularly by philosophers, is beside the point, and could have been avoided if it had been realised that his use of the term *a priori* was not the same as theirs. Eddington himself must bear the primary responsibility for this confusion. It is remarkable that the passage quoted has not been brought into the forefront of discussion. (Note added in proof: Recently my attention has been drawn to a valuable criticism by Harold Jeffreys, *Phil. Mag.*, 7th Series, 1941, 32, 177-205, in which Eddington's peculiar use of the words 'epistemological', '*a priori*', 'conventional' and 'subjective' is submitted to careful scrutiny.)

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our idea of physical space is necessarily three-dimensional in the sense that it could not conceivably have been anything else. Nevertheless, I regard Eddington's theory as 'the one notable exception' to the general verdict which I passed in my introductory paragraphs on previous attempts to tackle the problem.

(b) *The Dimensions of Space and the Forces of Nature.* The idea that the underlying characteristics of our conception of physical space are primarily conventional in origin was sustained by Eddington's contemporary and rival, Milne. Although Milne made no direct attempt to solve the problem, as Eddington did, I believe that he made a very useful indirect contribution. Milne's object in applying the technique of kinematic relativity to the problems of world-structure was to restrict the contingent elements in physics to an irreducible minimum. The concept of time was reduced to the observer's primitive awareness of time-order in his immediate experience. The concept of space was a construct, and Milne, like Poincaré, attempted to argue that its geometry was a matter of arbitrary choice. This was hotly disputed, and I believe that Milne's critics were more in the right than he was, but even Milne himself explicitly acknowledged that he assumed empirically that the number of spatial dimensions is three. He was driven to admit this, not so much because it was pointed out to him by his critics but rather because he realised that his attempt to deduce the inverse square law of gravitation as a property of matter in his world-model essentially depended on the number of spatial dimensions assigned to the model.¹ The fact that Milne was thus driven to explicit recognition of the purely contingent character of his assumption of three spatial dimensions was of great significance, since it served to re-emphasise at a deeper level than previously studied the intimate connection between the dimensions which we assign to physical space and our mathematical representation of the forces of nature.

The intimate connection between the inverse square law of force and three dimensional space has long been realised. Historically it can be traced back at least as far as the investigations of William Gilbert of Colchester on magnetic bodies. Like many other great men of science, Gilbert was not content merely to note the results of his experiments. He sought underlying explanations of phenomena. In particular he asked how it was that a loadstone could attract a piece

¹ E. A. Milne, *Proc. Roy. Soc. A*, 1937, 60, 8

of iron that is separated from it in space. To answer this question he reverted, like so many of the pre-Newtonian natural philosophers, to ideas which had been current in antiquity. In this case he went back to the type of answer which Thales accepted: magnetism was interpreted animistically. In Gilbert's view the Earth, since it was itself a great magnet, must have a magnetic soul and so must the loadstone. The power of this magnetic soul to act at a distance he explained by the hypothesis of a magnetic effluvium, emitted by the Earth on the loadstone, which reached out around the attracted body like a clasping arm drawing the body towards itself. This effluvium he regarded as spreading out from the soul spherically. 'For such is the property of magnetic spheres that their force is poured forth and diffused beyond their superficies spherically.'¹ Gilbert's views made a great impression on his contemporary, Kepler. Previously Kepler had believed that a tangential force was required to drag the planets around the sun. Gilbert, on the other hand, thought that a magnetic force similar to that exercised by the Earth on a compass needle caused both the Earth to rotate on its axis and the Moon to be dragged around the Earth. Kepler generalised this hypothesis. In his *Astronomia Nova* he wrote,

Since the Earth, as demonstrated by William Gilbert of England, is a great magnet and is rotated on its axis daily, so I believe the Sun to be rotated: and for this reason, because it has magnetic fibres intersecting the direction of its motion at right angles in the same way as those fibres surround the poles of the Earth in varying circles parallel to its motion: so I maintain with the best right that the Moon is whirled about by this rotation of the Earth and by the action of this same magnetic virtue . . . It is therefore plausible, since the Earth puts the Moon in motion . . . that the Sun puts the planets in motion by an emitted effluvium. . . .²

To these speculations we can trace Newton's idea of universal gravitation as a force which, in virtue of the three-dimensional character of space, varied inversely as the square of the distance and also Cavendish's similar law relating to electrostatic force.

The crucial significance of the number of spatial dimensions for the mathematical formulation of physical laws is beautifully illustrated

¹ W. Gilbert, *On the Loadstone and Magnetic Bodies and of the Great Magnet the Earth* (trans. P. Henry Mottelay), London, 1893, p. 304

² L. T. More, *Isaac Newton*, New York, 1934, p. 276

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by Laplace's equation. The inverse square law of force (attraction or repulsion) applies to individual particles. In the presence of any number of bodies which can be regarded as continuous distributions of particles, the equation

$$\frac{\partial^2 V}{\partial x^2} + \frac{\partial^2 V}{\partial y^2} + \frac{\partial^2 V}{\partial z^2} = 0,$$

where V denotes the potential, is valid at all points outside the bodies. *This equation is primarily an expression of the geometrical properties assigned to physical space.* Its algebraic form (sum of second-order derivatives) expresses the Pythagorean distance law of Euclidean space and the fact that there are three independent variables, x , y , and z , corresponds to the number of spatial dimensions postulated. It can easily be verified that an equation of Laplace's type in n independent variables would correspond to an inverse $(n - 1)$ th power law of force in a quasi-Euclidean space of n dimensions.

(c) *A New Suggestion for a Possible Solution.* The three-dimensional nature of physical space has thus been traditionally regarded as the reason for the predominating rôle played by the inverse square law in our analysis of the forces of nature. Assuming that, on the scale of the solar system, physical space can be regarded as approximately Euclidean—the Euclidean condition, equally with three dimensions, is an essential prerequisite for the inverse square law—let us now invert¹ the problem thus: presupposing the concept of lines of force, and hence assuming that a law of force proportional to the inverse s -th power of the distance is associated with (Euclidean) space of $n = (s + 1)$ dimensions, can we in any way account for the fact that the law of the inverse square, rather than of any other inverse power, predominates in physics, and so help to explain why physical space has just three dimensions and not any other number?

I suggest that a possible clue to the elucidation of this problem is provided by the fact that physical conditions on the Earth have been such that the evolution of Man has been possible. Is this possibility in any way dependent on the three-dimensional nature of space? (We know that in fact the motion of the Earth about the Sun is *primarily* governed by the inverse square law of attraction.) Now we know that during the five hundred or more million years in which

¹ A referee has kindly pointed out that the inversion of the problem was suggested by F. Exner (*Vorlesungen in die Naturwissenschaft*, Wien, 1921) and was later discussed by P. Frank (*Das Kausalgesetz*) and H. Reichenbach (*Raum-Zeit Lehre*).

organic evolution has taken place, the intensity of solar radiation on the Earth's surface cannot have fluctuated greatly, otherwise the higher forms of terrestrial life would have been destroyed and indeed might never have been possible. For this intensity to be approximately constant neither the Sun's rate of generation of radiation nor the Earth's distance from the Sun can have changed markedly during this immense period of time, since it is most improbable that a large variation in one would have been more or less compensated by any simultaneous variation in the other. Thus we believe that the Earth's distance from the Sun cannot have varied greatly, and hence we conclude that under prevailing conditions the Earth's orbit is not only very nearly circular but is also stable.

These considerations suggest that we examine the conditions under which a circular, or nearly circular, orbit in the Sun's field of force can be stable, without imposing at the start the customary condition that space is three-dimensional and hence that the motion of the Earth is dominated by an inverse square law of force. Instead, let us merely postulate that the Earth describes a nearly circular orbit about the Sun under the inverse s -th power law of force, appropriate to (Euclidean) space of $n = (s + 1)$ dimensions, and impose the condition that such an orbit is to be stable, so that as long as the Sun radiates steadily the intensity of radiation incident on the Earth's surface fluctuates little.

There is a well-known theorem of classical orbit theory which asserts that any circular, or nearly circular, orbit described in a central field of force with centre of orbit at centre of force is stable (against small radial disturbances) under a law of force proportional to the inverse s -th power of the distance if, and only if, s is less than three.¹ The proof of this theorem makes no demands on the number of dimensions which we must assign to space, except that this number must not be less than two.² Since we require $n = (s + 1)$ to be a positive integer, it follows that under the conditions now imposed the only admissible values of n are $n = 2$ and $n = 3$. We have thus discovered a possible criterion for eliminating all spatial dimensions *in excess of three*.

To isolate three-dimensional space as the only possibility for the

¹ H. Lamb, *Dynamics*, Cambridge, 1926, pp. 256-258

² The third dimension, orthogonal to the plane of the orbit, and equally any conceivable additional dimension, are irrelevant.

WHY PHYSICAL SPACE HAS THREE DIMENSIONS

world in which we find ourselves, we must now invoke some argument for showing why the number of dimensions cannot be *less* than three. To do this I suggest that we appeal to the geometrical structure of the higher forms of animal life. Topologically, a typical multicellular animal with an alimentary canal is a torus, i.e. a ring which does *not* divide the rest of space into two separated parts. No configuration of this type is possible in a two-dimensional space ; if space had less than three dimensions the highest form of animal life would either be unicellular or at most an aggregate of cells with a very different and presumably far more primitive structure than that of the human body.

To sum up : Since the mathematical discovery of higher space, a clear-cut problem has emerged concerning the origin of the three-dimensional character of physical space. Despite various recent attempts to show that this feature is either a necessary attribute of our conception of physical space or is partly conventional and partly contingent, the problem cannot be considered as finally solved. A new attempt to throw light on the question indicates that this fundamental topological property of the world may possibly be regarded as partly contingent and partly necessary, since it could be *inferred* as the unique natural concomitant of certain other contingent characteristics associated with the evolution of the higher forms of terrestrial life, in particular of Man, *the formulator of the problem*.

I should like to thank Professor H. Dingle for his valuable comments on a previous draft of this paper.

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ON THE OPERATIONAL INTERPRETATION OF CLASSICAL CHEMISTRY ★

JOHN BRADLEY

I *Introduction*

IN THIS article an operational analysis of the kind advocated by P. W. Bridgman is applied to the structure of that famous chemical argument, the atomic theory of Dalton and Cannizzaro. This theory is taken to include Dalton's atomic interpretation of the law of multiple proportions, Gay-Lussac's law of volumes, Avogadro's hypothesis, and Cannizzaro's method for the determination of atomic masses. It found confirmation in its extended and successful application by organic chemists and in Mendeléeff's periodic table. To Avogadro and Cannizzaro, as to Couper and Kekulé, the molecules and atoms considered in this great theory were real objects: they were thought of in the same way as one thinks of chairs and tables. In 1872 Cannizzaro himself said, with reference to his method of finding atomic masses and molecular formulae, 'that by this logical process the existence of atoms is deduced as a veritable law.'¹ Such realism, usually held uncritically, led the chemists who followed Kekulé to an understanding of the complex and subtle phenomena of molecular isomerism, including stereo-isomerism.²

Wilhelm Ostwald (1853-1932) attempted to restate the whole of fundamental chemical theory without the use of the atomic and molecular hypothesis and without the hypothesis of Avogadro. His book, *Prinzipien der Chemie*, published in 1907, employs the terms 'atom' and 'molecule' in footnotes only and is remarkable also for giving no examples. The theory of chemistry is presented in absolutely general terms, without what Ostwald had already described as the

★ Received 5.ii.54. Sections 3 and 4 appear, in an extended form, as part of ch. ix of my M.Sc. dissertation. (*The Ordering of the Concepts of Classical Physics and Chemistry*, 1952, University of London.)

¹ S. Cannizzaro and others, *Faraday Lectures*, Chemical Society, 1928, p. 35 (Cannizzaro, 1872)

² What follows may be regarded, in part, as a defence of critical realism in chemical theory.

unnecessary'¹ atomic hypothesis and without reference to any particular elements, compounds or mixtures.² Ostwald's purpose in the *Prinzipien* is partly to distinguish critically between what is given in experience and what is postulated by the mind;³ and partly to maintain that energy is the 'most general concept of the physical sciences'⁴ and therefore to formulate the first principles of chemistry in thermodynamical rather than molecular terms. The outcome is a presentation, positivist in general tone and very much in line with Mach's criticism of Newtonian mechanics and P. W. Bridgman's operational analysis of the concepts of physics.

2 Ostwald

Ostwald calls the transformation of a solution with two components into another phase a *hylotropic* transformation when, and only when, the two phases, e.g. liquid and gaseous solutions, have the same composition. Similarly he calls the solution of two components, which boils unchanged at a constant maximum boiling-point, a hylotropic phase. Such a solution is not normally regarded as 'a chemical individual' or compound, because its composition is a function of the pressure. In fact, experience shows that an increase in pressure may sometimes raise the proportion of component *A* in the maximum boiling solution and sometimes lower it, according to the nature of the other component *B*. When, in the special case, the proportion of *A* remains constant over a finite pressure range then the solution of *A* and *B* is also a chemical individual or chemical compound.

Therefore we conclude that a connection exists between solutions and chemical compounds or substances; the latter being a distinguishing case of the former . . . *a substance or a chemical individual is a body, which can form hylotropic phases within a finite range of temperature and*

¹ W. Ostwald and others, *Faraday Lectures*, Chemical Society, 1928, p. 188 (Ostwald, 1904)

² *Prinzipien der Chemie* is available in English: W. Ostwald, *The Fundamental Principles of Chemistry*, London, 1909. (Translation by Harry W. Morse)

³ Thus, he reminds us that experience alone does not reveal to us that a chemical compound contains its elements. We have first—say—mercury oxide, then mercury and oxygen; but these experiences do not compel us to affirm that the mercury oxide contained the two elements. Ostwald here follows Hume very closely: *ibid.*, pp. 256-258

⁴ W. Ostwald, *Natural Philosophy*, London, 1911, p. 56. (Translated from *Vorlesungen über Naturphilosophie*, Leipzig, 1902, by Thomas Seltzer)

pressure . . . we may infer from our definition that there exists a definite ratio between the components, independent of temperature and pressure between certain limits.

Now, *this is essentially the law of definite proportions*. . . .¹

The chemical elements are simply *substances which never form other than hylotropic phases*,² and Ostwald makes the bold conjecture that the stability of the elements, although very great, may be finite.

Now we are sure that for the transformation of one element into another, enormous amounts of energy would be required, for the concentrations of energy as yet available have proved themselves insufficient for this purpose.³

Ostwald proceeds to proofs of the laws of combining weights and multiple proportions which, in my view, are fallacious.⁴ In place of the usual concept of atomic mass he uses 'relative combining weight' and this is represented by a capital letter.

. . . it is possible to ascribe to each element a certain relative weight in such a way that every combination between the elements can be expressed by these weights or their multiples.⁵

Since the symbols AB and AB_2 or A_2B and AB may alternatively stand for the same pair of compounds, the 'choice of combining weights is an arbitrary one'.⁶

The freedom of choice which is left us in this case can be used for the purpose of expressing other relations, and, as a matter of fact, the volume relations of gases afford a means of making a definite choice between the various possibilities.⁷

It is thus clearly admitted that the 'chemistry without atoms' employs the same experimental generalisations as the classical theory.

Ostwald's form of Gay-Lussac's law is characteristically elegant.

The most general method of representing the amount of gas, independent of temperature and pressure, is given by the value of r in the gas equation

$pV = rT$, for in this equation we have

$$r = \frac{pV}{T}.$$

¹ W. Ostwald and others, *Faraday Lectures*, Chemical Society, 1928, pp. 194-195

² *ibid.*, p. 196

³ *ibid.*, p. 201

⁴ *ibid.*, pp. 197-198

⁵ *ibid.*, p. 197

⁶ W. Ostwald, *The Fundamental Principles of Chemistry*, London, 1909, p. 262

⁷ *ibid.*, p. 263

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In this form the gas law may be expressed as follows :

If two or more gases take part in a chemical reaction their r values are either the same or in a simple rational proportion.¹

He refrains from making the hypothesis of Avogadro and, instead, declares simply that the

. . . densities of various gases are in the same proportion as the combining weights of the gases, or simple multiples of them.²

This immediately suggests the possibility of making combining weight *strictly proportional* (or in appropriate units, numerically equal) to gaseous density. This is, however, impossible.

It is impossible for us to have combining weight and gas density equal, and at the same time to satisfy the requirement that the combining weight of compounds should be equal to the sum of the combining weights of the elements involved.³

The idea must therefore be given up. A new concept, that of *molar weight*, is immediately introduced.

. . . we gave up proportionality between gas density and combining weight, . . . We have retained the other principle by which combining weights of compounds are made up as the sum of the combining weights of their elements, premising in this that no fraction of combining weights shall appear in our formulae.

In order to express the law of gas volumes conveniently and simply, we have introduced a new concept to represent the weight of equal gas volumes, . . . *molar weights*.⁴

Thus Ostwald quickly loses the logical advantage which, for a time, he appears to enjoy over Avogadro. For he now has the *same number of concepts* as Cannizzaro and Avogadro, although he denies that they have molecular or atomic significance.

What follows⁵ is a translation of Cannizzaro's method into these terms, completely avoiding atoms and molecules, and advocating molar formulae both for elements and compounds.

[Some] elements never form gaseous compounds whose volume is greater than twice that of their own volume. It is therefore sufficient

¹ W. Ostwald, *The Fundamental Principles of Chemistry*, London, 1909, p. 265

² *ibid.*, p. 266

³ *ibid.*, p. 267. The traditional example is that of hydrogen chloride whose density is less than that of chlorine.

⁴ *ibid.*, p. 268

⁵ *ibid.*, pp. 268-271

to make the molar weight of such elements twice the combining weight in order to avoid fractions in the writing of formulae.¹

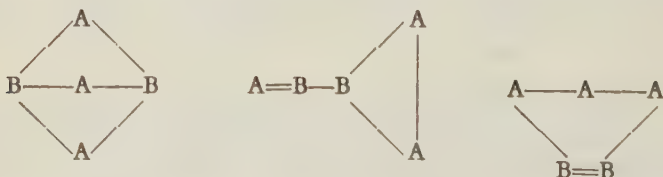
. . . the combining weight is so chosen that the coefficients necessarily applied to the element in the molar formulae of its compounds have no common factor.²

It is customary at the present time to write most of our chemical formulae so that they indicate, not only the combining weight, but also the molar weight. . . .³

When Ostwald turns his attention to isomerism he endeavours to characterise the phenomenon in terms of energy.

. . . isomeric substances can be defined as substances having the same composition but different energy content, . . .⁴

He notes that the term is sometimes confined to pairs of compounds whose molar formulae and weights are equal, and this important case is considered further. It is extremely difficult to believe that a mere difference in energy, which, as Ostwald admits, predicts 'nothing whatever about the chemical reactions which are to be expected',⁵ is sufficient to characterise and differentiate between dimethyl ether and ethyl alcohol. He is at length driven to the concepts of *valence* and *constitution*: in Ostwald's theory, it is the combining weight which has the valence, and the molar formula which is represented constitutionally. As an example of isomeric structures he gives :⁶



He has been obliged to admit alternative structures of *nothing in particular*! It is impossible to believe that A in the above diagrams represents 16 gm. of oxygen. What fraction of the combining weight does it represent? Either A represents an atom and the diagrams depict molecules; or the diagrams have no meaning.⁷

¹ W. Ostwald, *The Fundamental Principles of Chemistry*, London, 1909, p. 269

² *ibid.*, p. 270

³ *ibid.*

⁴ *ibid.*, p. 324

⁵ *ibid.*, p. 326

⁶ *ibid.*, p. 327

⁷ F. G. Donnan reports in his 'Ostwald Memorial Lecture' that in 'later years, owing to the work of Perrin and the newer developments of physics, Ostwald

3 Cannizzaro

To the logical mind of Ostwald it must have appeared paradoxical that facts, entirely macroscopic in character, should necessarily require an atomic interpretation; and that the hypothesis of Avogadro, which is certainly not a necessary inference from the atomic interpretation of the law of multiple proportions and Gay-Lussac's law, should turn out to be, what Nernst called it, 'an almost inexhaustible "horn of plenty" for the molecular theory'.¹ In the spirit of Ostwald, it is still of great interest to inquire, for example, how far *could* Cannizzaro and his successors conceivably have travelled without atoms and molecules and without Avogadro's hypothesis.²

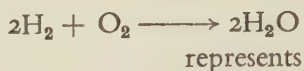
Let the density of hydrogen under standard conditions be defined as 2 units. The number is arbitrary. Let the densities of n gaseous compounds of hydrogen on this scale be experimentally determined as $d_1, d_2, d_3, \dots d_n$, all of which values are greater than 2. By gravimetric analysis the fractions by mass of hydrogen in the n gases are determined as $h_1, h_2, h_3, \dots h_n$. The products $h_1d_1, h_2d_2, h_3d_3, \dots h_nd_n$ are worked out. They are found to be a series of quantities of the kind 2, 1, 3, 2, 2, 1, 4, 2, 1, 4, 10, 1, \dots etc., all integral multiples of 1. The quantity 1 is *a part of a density* and is given some such name as *specific element density unit* and represented by a symbol H. Proceeding in the same way with sets of gaseous compounds of other elements it is at length established as an empirical rule that gaseous densities can be built up from whole numbers of specific element density units. The specific units are represented by the symbols C, N, Cl, H, O, S and the rest. A *density* such as that of methane is represented by the symbol CH_4 , made up from

somewhat modified his views and admitted the existence of an apparently discontinuous "grainedness" in the physico-chemical world—in other words, atoms and molecules'. See F. G. Donnan and others, *Chemical Society Memorial Lectures, 1933-1942*, Chemical Society, 1951, p. 12.

¹ W. Nernst, *Theoretical Chemistry*, London, 1895, p. xiii. (Translated by C. S. Palmer)

² After I had worked out the argument which follows I found that Dr R. A. R. Tricker, also inspired by Professor Bridgman's ideas, had been thinking along very similar lines. I am indebted to Dr Tricker for discussing the matter with me very fully although I alone am responsible for the form of the argument and the conclusions drawn.

one carbon unit and four hydrogen units. A chemical relationship of the kind



2 volumes of hydrogen of density 2 + 1 volume of oxygen of density 32 \longrightarrow 2 volumes of steam of density 18.

All this, without atoms, molecules, and Avogadro's hypothesis, is a *possible* achievement. It is instructive because it shows to what a remarkable extent chemists did multiply entities unnecessarily in chemical theory. For the most important part of his argument Cannizzaro had two names for the same thing, *volume* and *molecule*. Nor does this exhaust what could have been done without the hypothesis of Avogadro. The laws controlling the numbers of specific units in the *densities* CH_4 , H_2 , HCl , Cl_2 , CH_3Cl , H_2O , CO_2 could have been formulated as principles applying to *densities*. Even the term 'valency', with no atomic reference, could have been suggested.

The study of the composition of solid and liquid substances could also have been developed without molecular and atomic theory. In Cannizzaro's *Sketch*¹ the elements solid bromine and iodine are used to *calibrate* the heat rule of Dulong and Petit for solid crystalline elements.² Knowing the *specific element density units* of two elements by the standard method, it would be theoretically possible to derive *density units* for the metals from a law of the Dulong and Petit type and accurate gravimetric analysis, but the law would need to be reformulated. A formula such as NaCl would represent the composition of common salt and would also represent a sub-multiple of its density as a gas. It was an error of the nineteenth century to think of NaCl necessarily as a molecule ;³ perhaps, if the whole theory had been developed without molecules, the *corresponding error* would have been less often made. Again, when Raoult discovered the

¹ S. Cannizzaro, *Sketch of a Course of Chemical Philosophy*, 1858 (See Alembic Club Reprints, No. 18, pp. 22-24)

² In 1819, Dulong and Petit had no correct way of finding the supposed constant value of the gram-atomic heat of a solid crystalline element. There was required at least one atomic mass of such a solid, determined by a method entirely independent of the heat rule. This, and more, was provided in 1858 by Cannizzaro.

³ S. Cannizzaro and others, *Faraday Lectures*, Chemical Society, 1928, p. 38. Cannizzaro did *not* make the error.

relationship between the depression of the saturated vapour pressure of a solvent and the molecular mass of a relatively non-volatile solute, he used, first, solutes of molecular masses known from Avogadro's hypothesis.¹ Conceivably, the discovery could have been made that equal concentrations of *density* units produce equal depressions of saturated vapour pressure. A formula such as $C_{12}H_{22}O_{11}$ would represent an amount of sucrose producing a standard vapour pressure or boiling-point effect; and also a possible hypothetical density of sucrose vapour. The reaction between equal volumes of ammonia and hydrogen chloride,



would continue to be represented in this way; where NH_3 and HCl are not molecules, the error of taking NH_4Cl to be one would not be made. The corresponding error would be to regard NH_4Cl as necessarily a density. Experiments on the boiling-points of ammonium chloride solutions, with other evidence, would lead, not to the recognition of ions as such, but to the recognition of *species* NH_4^+ and Cl^- . It is true that 'ammonium ion' is just as much a compound species as 'methane'.

Let us suppose that it so happened chemical theory did progress along these lines without the hypothesis of Avogadro. Would it have continued in the same way to the present time, a sheer alternative to Cannizzaro's system? It seems very unlikely. Sooner or later, it would have been realised that molecules are an immense asset in chemical explanation. The peculiar facts of isomerism alone would have been a powerful factor driving chemical theory in the direction of molecules. For example, let us suppose that in 'chemistry without Avogadro' it is found there are two and only two different substances whose densities are equal and composed of the same density units $C_2H_4Cl_2$. The facts are not 'explained' by formulating the density of ethane as CH_3CH_3 instead of as C_2H_6 , and the densities of the two chlorine compounds as $CHCl_2CH_3$ and CH_2ClCH_2Cl . *Purely* as density formulations these do not differ; it makes no difference to a sum whether one adds up from the bottom or down from the top. One has still to inquire why there are two substances whose densities are both represented by $C_2H_4Cl_2$. If the two equal densities represent equal numbers of molecules or 'little objects' in the

¹ F. M. Raoult and others, *The Modern Theory of Solution*, New York and London, 1899, pp. 105-110. (Memoir by Raoult, 1888, translated by H. C. Jones)

given standard volume then the arrangements CHCl_2CH_3 and $\text{CH}_2\text{ClCH}_2\text{Cl}$ may stand for the composition by atoms of molecules of equal mass. The isomerism is explained by the molecular hypothesis used in a geometrically realist fashion, and Avogadro's hypothesis is taken back into chemical theory, which thereby has its historical quality restored to it.

It appears then that chemical theory *could* have made substantial progress in important directions without its molecules although such progress would have been, in my view, greatly retarded. Also, sooner or later, the need for molecules would have been so sharply felt that they would have been introduced after all. The application of operational analysis to the most famous chemical induction lays bare and clean the nature of the argument and is therefore valuable ; but it does not enable the chemist to dispense with his favourite concept, the molecule with structure.

The following section gives a further illustration of this thesis and shows how chemists have insisted on regarding the molecule in the same way as, and by extended analogy with, the common object.

4 *Victor Meyer*

The inadequacy of a planar arrangement for the valencies of the carbon atom in the molecule of methane was also recognised about 1871 by Victor Meyer, who realised that such an arrangement would involve the existence of two isomeric methylene chlorides.¹

When 'combining capacity' was first discovered (1858), the valency bonds had only two properties, definiteness of number for each kind of atom, and the power to link atoms. Crum Brown (1864) was justified in writing the 'double bond' in the ethylene molecule

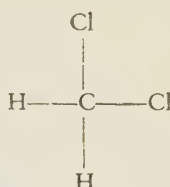
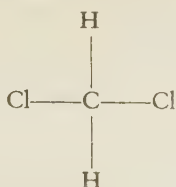


because whenever this new symbol was necessary the corresponding compound had the property of removing the colour of alkaline permanganate. The 'double bond' was associated with an additional property and the new symbol was therefore logically justified. But there appears to be no sort of right for a chemist in 1871 to have assumed gratuitously that the hydrogen atoms are squarely set about

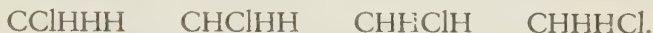
¹ A. J. Berry, *Modern Chemistry*, Cambridge, 1946, p. 64

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the carbon atom and that therefore there should be two isomeric dichloromethanes :



Equally, Victor Meyer could have looked for four or more isomeric monochloromethanes formulated linearly :



From the point of view of Mach and Ostwald the concept of valency has the attributes its creators choose to confer upon it. Victor Meyer 'should' have been content with CH_4 and CH_2Cl_2 in which the four valencies of the carbon atom are satisfied in two and only two different ways. He was 'wrong' even to wonder why there is only one dichloromethane. Having failed to find the two isomers, he 'should' have been repentant and learnt the lesson that a scientific concept is merely an artifact of the mind bearing only the attributes of its original endowment.

What Victor Meyer did was very different. The idea of molecular structure suggested to his mind, as it had already done to the mind of Kekulé, a wealth of possible implication, an evolutionary view of the articulated molecule. A carbon atom in a methane molecule was to him no more likely to have merely four bonds *and nothing else*, than a chair was likely to have four legs and nothing else. There is no doubt at all that Victor Meyer was provisionally prepared to consider that the bonds from the carbon atom must have a definite spatial arrangement and a definite length although such refinements of the concept of valency were not present in Cannizzaro's *Sketch*.¹ Victor Meyer was led to these refinements by thinking of the methane molecule in the same way as one thinks of a chair. The legs of a chair are arranged in a certain way which excludes others ; the bonds from the carbon atom may be directed to the corners of a plane square and, if so, this arrangement excludes others.

The absence of two dichloromethanes in no way curbed such thinking ; it led to an even bolder speculation, that of the recognition

¹ S. Cannizzaro, *Sketch of a Course of Chemical Philosophy*, 1858 (See Alembic Club Reprints, No. 18)

of the three-dimensional character of the molecule object. That remarkable branch of chemistry, called *La chimie dans l'espace* by van t'Hoff, shows that Victor Meyer was right.

5 Conclusion

In the *Prinzipien* Ostwald does not admit that the facts of isomerism spell the defeat of his enterprise although his account exposes very honestly the immense difficulties to which he has been led. The less extended operational analysis of Cannizzaro's method which I have attempted here cannot be sustained without the reintroduction of, first, Avogadro's hypothesis and, later, the 'little objects', made up of atoms and constructed in very special and precise ways. And again it is isomerism which compels the student of chemical theory to these conclusions. Evidently the complex facts which led to the atomic and molecular theories do need such theories. Positivist and operational analysis is, however, valuable because thereby the logical character of a scientific theory may be more truly exposed and the theory purified from material which is of historical interest only. The present study, for example, does I think attribute to the facts of isomerism a logical significance which is liable to be missed by the student of the history of chemistry.

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A CLOSE-CLIPPED VIEW OF THE UNIVERSE, WITH A NOTE ON ITS 'ORIGIN' AND 'AGE', AND OUR POSITION IN IT *

H. W. POOLE

'I was wondering what the mouse-trap was for', said Alice. 'It isn't very likely there would be any mice on the horse's back.' 'Not very likely perhaps', said the Knight, 'but if they *do* come, I don't choose to have them running all about.'

THERE is a remarkable passage in Schopenhauer's *On the Fourfold Root of the Principle of Sufficient Reason*¹ which, if true, appears to lead to far-reaching consequences. This is what he says :

Our knowing consciousness, which manifests itself as outer and inner sensibility (or recëptivity) and as understanding and reason, subdivides itself into Subject and Object and contains nothing else. To be an Object for the Subject, and to be our idea or our representation, are the same thing. All our representations are Objects for the Subject, and all Objects for the Subject are our representations. Now it is found that all our representations stand towards one another in a law-like connection, the form of which can be determined *a priori*, and owing to which nothing existing separately and independently, nothing single or detached, can become an Object for us.

So far as I know, this paragraph has not been much discussed in philosophical literature.² One writer, Jules de Gaultier, made it his life's study, and he wrote some twelve volumes round it.³ He was hardly known in England,⁴ and seems now to be completely forgotten

* Received 27.xi.53

¹ 1813, 1847; in the English translation, Schopenhauer, *On the Fourfold Root of the Principle of Sufficient Reason*, London, 1897, Ch. III, § 16, p. 30, in the important passage here quoted a sentence of the original was omitted. Professor K. R. Popper kindly drew my attention to this, and the omission has been rectified above.

² But see H. Dingle, *The Scientific Adventure*, London, 1952, pp. 236-255

³ Jules de Gaultier, *De Kant à Nietzsche*, Mercure de France, 1900, 4th ed., 1910

⁴ Havelock Ellis, *Impressions and Comments*, London, 1920, pp. 206-208. Algar Thorold, *Six Masters in Disillusion*, London, 1909. See especially the Epilogue, pp. 147-163. This book, to which I am much indebted, was my introduction to de Gaultier's writings.

in France.¹ I have no wish to deny my debt to him ; rather would I draw attention to it.

If it be true that the faculty of knowing involves essentially a representation of an object for a subject, this does not quite conform with accepted ideas. It is currently assumed that there is a Universe in space and time, which (to speak personally for heuristic reasons) is independent of me : that I have a body which is dependent on the Universe : and a mind which is dependent upon the body. I may apply my mind to the study of the Universe, of my body, or if I wish, of my mind. If, however, I so apply my mind, I find this to be happening : the Universe, my body and my mind, as I become aware of them, are no longer separated and individual. As I have knowledge of them they are joined by a relationship, that of being an object presented to a subject, a state of affairs which cannot be eliminated. The Universe *as known*, the body *as known*, and the mind *as known*, are only given in this relationship : that of an object for a subject. When I apply my mind to the study of my mind, it is *only* this relationship that I encounter. Indeed, what we call 'Universe', 'body' and 'mind' seem to have no separate or individual existence of which we can ever become aware. Their existence for us may be said to *be* just this relationship. It would be tempting to step outside this relationship ; to ascertain exactly what is related to what : tempting, but impossible. To do so would be to transcend the relationship which is precisely what (it would seem) we cannot do. Schopenhauer's words are plain and will bear repeating : ' . . . nothing single or detached can become an object for us.' We may find a phrase from the Creed of Saint Athanasius a help.² On pain of being seriously confused we must not confound the 'persons' (object and subject) nor divide the 'substance' (the relationship).

Kant was dimly and uneasily aware of the situation that Schopenhauer later was to bring to light. Kant's well-known and decisive refutation of the arguments to prove the existence of God alarmed the great thinker (for intellect and temperament are sometimes an ill-assorted pair) ; so in his *Practical Reason* he welcomed back to his empty heart the Deity he had denied in his first *Critique*. The implications of Schopenhauer's work went deeper : not only were there no grounds for a belief in God, there were no grounds for a belief that we had

¹ Not mentioned in *Philosophical Thought in France and the United States*, ed. Marvin Farber, New York, 1950

² *Book of Common Prayer*, At Morning Prayer, *Quicumque Vult*

knowledge of His Universe either. Schopenhauer himself, sceptical enough about The Creator, never, I think, actually drew the second conclusion. *The World as Will and Idea* (1818) was written in almost complete disregard of his earlier book (1813). Is it not noteworthy that Christian and unbeliever alike boggled at the plain meaning of their own thought, and ran from it?

It only remains to be said that the Kantian intuitions of space and time, the causal chain (with, however, no first nor last link), the judgments, and the categories are the lenses through which the faculty of knowledge presents an object to the subject.¹ The disciplines of logic and mathematics should keep these lenses clean, and exhibit, where possible, the structure of their juxtaposition. These auxiliaries are, in theory, the only exact sciences, and their province is truth.

Looking at the physical and mental sciences of his day, Jules de Gaultier remarks with some irony, that their province was the cultivation of mystery.

The object of science [he observes] is the cultivation of mystery. To understand scientifically is to become more susceptible to astonishment. A profession of agnostic faith is the basis of all scientific thought worthy of the name. A scientist reveals the measure of his mind when he thinks the indefinite progress of science, not as the discovery of truth, but as a more direct view and the more intense sensation of mystery.²

That was written in 1900; but is it any the less an exact description of our present situation? It is small wonder that so many philosophical and scientific books sparkle with quotations from *Alice in Wonderland*. To their authors the study of this tale is a busman's holiday.³

Classical physics was disrupted because Nature did other than was expected of her; then refused answers to questions which appeared reasonable; and finally forbade proposed experiments absolutely. It is hardly necessary to give examples: radiation from a black body, the Michelson-Morley experiment, and the attempt to give precision to both position and velocity of a particle, come to mind.

This situation was changed by two expedients; the introduction of the observer, and the employment of better equipped mathematicians.

¹ Kant, *Critique of Pure Reason*, trans. N. K. Smith, London, pp. 65-81, 105-119

² de Gaultier, op. cit. p. 122

³ *The Philosophy of Mr B*tr*nd R*ss*ll*, ed. P. E. B. Jourdain, London, 1918, pp. 89-96

The eventual appearance of an observer might have been foreseen, since 1813, by anyone who had read Schopenhauer's book. This in itself is an impressive tribute to the power of his principle. The scientist as an active observer has lifted a little of the mystery, but the mathematicians have themselves produced a good deal more. To see how this has happened we must understand the nature of the mathematical crisis.

This most exact of the sciences reveals itself as in some confusion. In the first place, there is no consensus of opinion as to the nature of mathematics. Is this enormous and impressive mass of intricate and exquisite beauty an invention or a discovery? ¹ That the swords which are fashioned do exploits, we know; but what sort of exploits? Are these weapons chimerical excaliburs transfixing ghosts; or cold steel in flesh and blood? Like Pilate, we do not stay for an answer: for nobody knows it. The mathematicians themselves are uncertain. The finest minds of our time are divided almost equally in three irreconcilable schools. ² In the second place, in spite of great efforts, long continued, arithmetic has not yet been established on a logical basis. K. Gödel in 1931, proved, in a remarkable manner, that it is impossible to construct a formalised logical system which includes all true arithmetical propositions. ³ Berkeley, who defended free-thinking in mathematics in 1735, ⁴ after some 200 years appears to have a formidable successor. Mathematics is the last stronghold of ontological proofs. ⁵ Until it has been shown how thought can produce being, the attempt to construct a Universe out of non-euclidean geometry, logarithmic scales of time, and anti-commutating matrices, will result in wraiths. The value of these beautiful mental constructions is that they allow us to organise that relation of subject-object which is the only reality for us. Eddington's rediscovery of the constants of nature by calculation, at once a stumbling-block and an awe-inspiring and magnificent feat, was really to be expected once Schopenhauer's principle had been thought of. The constants are not *in nature* at all: they are *in the relation*; and that relation, apparently, can be attended to from the end of the subject or from the end of the object. Those

¹ See G. H. Hardy, *A Mathematician's Apology*, Cambridge, 1940, pp. 63-64

² Max Black, *The Nature of Mathematics*, London, 1933

³ K. Gödel, 'Ueber formal unentscheidbare Sätze der Principia Mathematica und verwandter Systeme', *Monat. für Math. und Phys.*, 1931, 38, pp. 173-198

⁴ Berkeley, *Works*, ed. Luce, Edinburgh, 1951, Vol. 4, pp. 109-138

⁵ E. W. Beth, *Les fondements logiques des mathématiques*, Paris et Louvain, 1950

philosophers who distrusted the proceedings were, however, backing the same horse. This was not noticed at the time for the animal from tail to head looked strangely different when viewed from head to tail.¹

By virtue of Schopenhauer's principle we assert that neither a separated God nor the external Universe can become an object of knowledge for us. We cannot affirm that these entities do not exist : but merely that we cannot know them. We all live in a common world that may be called real, although each of us has a private view of it. It is usual to think that this common world has an impersonal and objective basis ; and that by some mental process we can arrive at an understanding of what it is ; and that this is the world with which science deals. This procedure violates all the canons of exact thinking, for, could this manoeuvre really be carried out, we should be left with a separated and detached entity : in a word, with a subjectless object. The history of science, since at least 1900, shows us that this is a position daily more difficult to maintain. We add epicycles to cycles to keep theory in step with facts. In our modern science the sun still goes round the earth : the scientists stand in submission before their apparatus. But the experimenters are on the sun ; and the only world they will ever know revolves round *them*. It may be worth their while to reopen the Michelson-Morley experiment in the light of Schopenhauer's idea.

Further consequences follow, I believe. The philosophy of the detached object, realism ; and that of the separated subject, idealism ; are not partial views of the truth, but serious distortions of it. The body-mind problem emerges from a new angle. Nominalism stands four-square. The Platonic ideas and forms, with the Aristotelian universals (the backbone, not only of medieval realism, but of all theology that aims to incorporate reason, and indeed, much of modern logic) can no longer be effectively defended. Mathematics, taken from the clouds, will be handed back to its gifted makers : they even constructed the integers. We may expect the hedges and thickets, grown by the practitioners of symbolic logic to be somewhat pruned : and while the writers on logical positivism will no doubt continue their interminable monologues, they will find, I hope, that certain of their subjects have dried up. Schopenhauer's Razor is sharper than Ockham's, for it has made smooth the Platonic chin. This does not

¹ Eddington, *Fundamental Theory*, Cambridge, 1946

seem to be generally known. We find, surprisingly enough, W.V.O. Quine writing thus : ' I have spoken disparagingly of Plato's beard, and hinted that it is tangled. I have dwelt at length on the inconveniences of putting up with it. It is time to think about taking steps.' ¹ It will be seen that the great logician is on the side of the angels even if he has not yet quite caught up with them.

We are presented, then, with a pared Universe ; one that fits into the scheme of our comprehension, because from the matrix of that comprehension we and It alike are the elusive and enigmatic issues. The ineluctable mystery of things lies beneath the equal mystery that is our own awareness of ' the common round, the trivial task ' ² We are obliged to think of the Universe as a Thing, in the flux of Cause and Effect, endowed with an Existence, in Space and Time ; for it is only through these Kantian intuitions and categories, these lenses, as I have called them, that an object can be presented to a subject. If, however, we take this cryptic relationship too literally and ask, for example, if the Universe has *origin* and *age*, we shall enmesh ourselves in paradox. It will be instructive to observe how such an investigation *must* proceed.

As scientists we are too scrupulous to see the origin of the Cosmos, as the theologians do, in the Fiat of a God. No Spirit moves for us upon the face of the waters. ³ Yet this Primeval Atom is there for explanation, Canon Lemaître having kindly swept the expanding galaxies back into It for our inspection. ⁴ This Atom, we say, must have been the effect of a previous cause. The principle of causality forces us to seek the cause of that cause, and to mount up indefinitely in the void, from cause to cause. ⁵ If we say, ' better stop at the Atom ', then we cease to play the intellectual game, and shoot the umpire. ⁶ If the unobservable and the unverifiable are to be ruled out on other grounds, it is clear this Atom is a very unscientific one ; unless it should be said that the origin could have been observed and verified ' in principle '—a phrase to which A. J. Ayer ⁷ allows considerable elasticity. Now, this is interesting, because the words ' in principle ' harbour a homunculus ! That is to say a *subject* is *invented* to make

¹ W. V. O. Quine, *From a logical point of view*, London, 1953, p. 5

² John Keble, *The Christian Year*, morning

³ Genesis I, ii

⁴ G. Lemaître, *The Primeval Atom*, London, 1950

⁵ de Gaultier, op. cit. p. 73

⁶ For an example of this murder see E. A. Milne, *Modern Cosmology and the Christian Idea of God*, Oxford, 1952, pp. 96 and 159-160

⁷ A. J. Ayer, *Language, Truth and Logic*, London, 2nd ed., 1947, pp. 9-11, 36

VIEW OF THE UNIVERSE

sense of the object. Canon Lemaître's book is delightful, mathematical, scientific, the finest flower of the disciplined imagination ; but I fear Schopenhauer's canon of thought has destroyed the thought of the Canon. We cannot in fact visualise the beginning of the Universe without anticipating its evolution in several particulars by giving ourselves ringside seats to watch the spectacle. Yet when we get there any time between the years unanimously recommended by the astronomers, namely, B.C. $3 - 6 \times 10^9$, we find we are already hopelessly late, and that the really important moves took place before we arrived.

Our own space is small, and our time short. Are not thousands of millions of years an audacious extrapolation beyond the edges of our tiny graph ? It is only because *we* are born that we can speak of origin ; and only because *we* die that we can speak of age. Were we immortal we could not *understand* such words.

No matter : Time is now regarded as an objective entity. It is long-suffering. The mathematicians make it go backwards and forwards like a palindrome, or a canon cancrizans. They make it hurry, dawdle, stand still, and twist it. It is no longer single, having been married to Space. The late S. Alexander wrote in two volumes a remarkable epithalamium upon the event ;¹ and many and wonderful universes have been constructed from the marriage lines. We taste the dusty and ashy flavour of Time : must we crown the Universe with the signature of our own mortality, and cram it full of years, though they be counted five times over, when we have none to spare ? Time is a lens by which our common world is represented for, and in, us. It is a tiny and fragile instrument, and serves for not much more than three score years and ten. Yet to understand what we *mean* by an age of three times a million millenniums we must tell the lessening residue of our own days for a gauge.

Over our questions there has presided an unacknowledged guest : abolish him, and the universe has no origin, no age and no meaning.

Again I quote the words of the strangely neglected French master : ' True philosophy understands the power and limits of the mind, does not confound its categories, nor ask of it more than it can give.'²

How then : Are we not as those having no hope and without God in the world ?³ I do not think so. The desire to be good may

¹ S. Alexander, *Space, Time and Deity*, London, 1920

² de Gaultier, op. cit., p. 124

³ Eph. 2, xii

be satisfied if we do justly, love mercy, and walk humbly.¹ The kingdom of heaven, said to be within us,² and that deity, in whom, perhaps, we live and move and have our being,³ if known at all, are known within that relationship which alone is our reality. Wittgenstein has said 'Death is not an event in Life',⁴ and within the ambit of his own self-imposed narrowness of expression, he was right. Yet death is almost the *only* event in life. Saint Paul has told us that he died daily.⁵ It was an understatement. We die every second of the three thousand millions of them that may be our portion. The child is coffined and interred deep in the young man; and the young man is disintegrated and dust in the old man. We are ossuaries all, valleys of dry bones;⁶ we live among the tombs, and our name is Legion.⁷ We cannot evade the relativity of our own experience; that inexorable winding-sheet, stamped at the ends with the leaden seals of birth and death. The reaper does not stand before us, but walks with us, ever cutting at our heels swathes from the wheat-field of our lives, and leaving them to be collected by us into the store-houses of our memory. Is it true that 'he that goeth on his way weeping bearing precious seed shall come again with rejoicing, bringing his sheaves with him'?⁸ Deep in our Platonic cave⁹ we see through a glass darkly;¹⁰ and we can give no answer. But many good men and true have closed, for the last time, their eyes in a sure and certain hope.¹¹

¹ Micah, 6, viii² Luke, 17, xxi³ Acts, 17, xxviii⁴ L. Wittgenstein, *Tractatus Logico-Philosophicus*, London, 1922, 6.4311⁵ 1 Cor., 15, xxxi⁶ Ezek., 37, i-ii⁷ Mark, 5, ii, ix⁸ Psalms, 126, vi⁹ Plato, *Republic*, Book VII¹⁰ 1 Cor., 13, xii¹¹ *Book of Common Prayer*, Burial of the Dead.

The view outlined seems to me to have some affinity but not identity with the philosophy of Professor Dingle, although he may repudiate this suggestion.

In many places I have unhesitatingly followed Kant; and said so. He may not have been in full possession of Schopenhauer's principle, but he nevertheless came very near it. His theory of the problem of the beginning of the Universe, though more complicated, is no doubt essentially similar to that which I was led to in developing the implications of Schopenhauer's principle.

It will be found that P. C. Jones (*Phil. of Sci.* Balt. 1949, 16, 49-57) has produced, on a somewhat *ad hoc* foundation, a system closely similar to that of my essay. I did not find his article until I had concluded mine.

My essay owes much to my friend, J. Mills Whitham, novelist and historian. His *Windlestraw* set me thinking on the meaning of tragedy. His unpublished philosophical work, *Recipience*, acted as a spur to my scepticism.

NOTES AND COMMENTS

Two Autonomous Axiom Systems for the Calculus of Probabilities

I Informal Explanations

I SHALL introduce, in this paper, two ideas. The first is that of absolute probability :

$$p(x) = r \quad (\text{A})$$

which may be read 'the (absolute) probability of x equals the real number r '.

The second is that of relative probability :

$$p(x, y) = r \quad (\text{R})$$

which may be read 'the probability of x given y equals the real number r '.

Both $p(x)$ and $p(x, y)$ are numerical functions of non-numerical variables or arguments. *The arguments may be interpreted in various ways.* Thus ' x ' and ' y ' may be interpreted as (variable) *names* : for example, names of statements, or of classes, or of predicates, or of events.

My intention is to construct a system that is *formal* in the sense that *its interpretation is left open*. This does not mean that in constructing the system I have no interpretations in mind—on the contrary, I have a considerable number of *different* interpretations in mind.

Moreover, the system is *autonomous* in the sense that it is designed in such a way that it allows the calculation of probabilities, but does not assume a calculus for operations with the arguments. What is meant by this may be explained as follows.

Most writers on the subject assume, before proceeding to give their axioms for probability, that certain laws are valid for their arguments, for example, the commutative and associative laws :

$$xy = yx \quad (\text{I.1})$$

$$(xy)z = x(yz) \quad (\text{I.2})$$

Assuming substitutivity of identical terms, they derive from these

$$p(xy) = p(yx), \quad (\text{I.1}^a)$$

$$p((xy)z) = p(x(yz)), \quad (\text{I.2}^a)$$

and similar equations for relative probabilities. It is usually assumed that all the rules of Boolean algebra hold for the arguments, or that the rules of the calculus of propositions hold for them.

Since I am interested in constructing *systems in which not more is assumed than a bare minimum necessary for the calculus of probabilities*, I shall not make any such assumption as I.1 and I.2 but shall instead use axioms of the

form 1.1 and 1.2^a. These are very much weaker ; for from 1.1 and 1.2 we can obtain, as indicated, 1.1^a and 1.2^a by substitution, but not the other way round. A system in which only formulae such as 1.1^a and 1.2 are used and no formulae for the arguments alone, I call an 'autonomous system for the calculus of probabilities'.

Some authors believe that only the idea of relative probability makes sense, and that absolute probability is meaningless. But after introducing autonomously ' xy ', interpretable as the conjunction or meet of x and y , and ' \bar{x} ', interpretable as the negation or complement of x , it is always possible to define absolute probability in terms of relative probability by a definition like

$$p(x) = p(x, \bar{x}) \quad (1.3)$$

This definition gives meaning to absolute probability, in terms of relative probability.

Similarly, we can define relative probability in terms of absolute probability, by a customary definition like

$$\begin{aligned} \text{Let } p(y) \neq 0; \text{ then} \\ p(x, y) = p(xy)/p(y) \end{aligned} \quad (1.4)$$

This defines relative probability provided $p(y) \neq 0$; for $p(y) = 0$, relative probability is undefined. This is very awkward, especially if we intend to allow for a *logical interpretation* of the calculus. For in such a *logical interpretation of the formal system* we wish to have a rule like the following *rule of interpretation* :

$$\text{If } x \text{ follows from } y, \text{ then } p(x, y) = 1.$$

And this rule (which does not belong to the autonomous calculus but to one of its interpretations) should hold quite independently of any assumption as to $p(y)$ not being zero, for at least two reasons. First because, if y is a universal law and x a singular instance of it, then $p(x, y)$ should be 1, even if $p(y)$ equals zero. Secondly because $p(y\bar{y}) = 0$; but since every x follows from $y\bar{y}$, we wish $p(x, y\bar{y})$ to be equal to 1. For these reasons, 1.4 is not quite adequate as a definition of relative probability in terms of absolute probability ; and the same can be said of

$$p(x, y)p(y) = p(xy) \quad (1.5)$$

which is easily seen to be equivalent to (1.4.) On the other hand, it would be quite inadequate to postulate¹ that, if $p(y) = 0$, $p(x, y)$ should always be 1. For take the case of a universal law y with $p(y) = 0$, and of an x

¹ This is a possibility discussed and rejected by Carnap, in *Logical Foundations of Probability*, 1949, p. 295 sq. Carnap's reasons for rejecting it are quite different from mine.

TWO AUTONOMOUS AXIOM SYSTEMS

which contradicts y . In such a case we wish $p(x, y)$ to be zero rather than one, even though $p(y) = 0$.

I shall give below a definition of $p(x, y)$ in terms of $p(x)$ which gets over these difficulties. We can easily interpret this definition by purely universal and purely existential sentences. But if we wish to extend the method in order to interpret the calculus by sentences with mixed prefixes of universal and existential operators, then it soon becomes unwieldy, to say the least.

For such purposes it is preferable to build up the calculus as a calculus of *relative probabilities*, and to introduce absolute probabilities by a definition such as (1.3), or some equivalent formula.

In what follows, I shall give two systems, one that takes absolute probability as fundamental, and one that takes relative probability as fundamental. Both systems are demonstrably consistent and independent.

2 A Formal System of Absolute Probabilities

Postulate 1. If x is an element of the system S , then $p(x)$ is a real number.

Postulate 2. If x, y and z are elements of the system S , then xy and \bar{x} are elements of the system S , and the following *axioms* hold :

- | | | |
|-----|--|------------------------------|
| A.1 | $p(xy) \geq p(yx)$ | (Commutation) |
| A.2 | $p((xy)z) \geq p(x(yz))$ | (Association) |
| A.3 | $p(xx) \geq p(x)$ | (Tautology) |
| B.1 | $p(x) \geq p(xy)$ | (Monotony) |
| B.2 | $p(x) = p(xy) + p(x\bar{y})$ | (Complement) |
| B.3 | $(x)(Ey)(p(y) \neq 0 \ \& \ p(xy) = p(x)p(y))$ | (Existence & Multiplication) |

This last axiom ¹ demands for every element x the existence of an element y which has a probability $\neq 0$ and which is independent of x . At the same time, it is clearly a much weakened consequence of (1.3) and (1.4), since it demands for every x a y such that $p(x) = p(xy)/p(y)$, that is to say, in view of (1.4), such that $p(x) = p(x, y)$. If (1.3) and (1.4) are admitted

¹ It replaces A4 and B3 of my old system in *Mind*, 47 (N.S.), 1938, p. 275 sq. However, B3 of the present system may be split up (as in the note in *Mind*) into two :

- | | |
|------|---|
| B.3- | $(x)(Ey)(p(y) \geq p(x) \ \& \ p(xy) = p(x)p(y))$ |
| E.1 | $(Ex)(Ey)p(x) \neq p(y)$ |

Axiom B.3- (Multiplication and Independence) corresponds to B.3 and C.2 of the relative system. I may perhaps say here that I still uphold the views of my note in *Mind*, except that I now regard the relative system as preferable to the absolute system : at the time I believed that a postulate 1 (as I call it here ; in my note in *Mind* it is called, most misleadingly, 'Axiom of Uniqueness') could not, without contradiction, be formulated for the relative system as strongly and unconditionally as for the absolute system.

to be intuitive, then, clearly, all axioms of the system are intuitive. This is interesting in view of the fact that the system allows the deduction of the laws of the upper and lower bounds which have been often asserted to be conventional (or non-intuitive¹).

In conjunction with (1.4), our system allows the derivation of all the formulae of the customary systems of probability. However, I aim at a definition which is stronger than (1.4).

In order to define $p(x, y)$ on the basis of these axioms, I make use of the concepts of a finite system; of a sequence of finite systems S_n ; and of the limit system of this sequence.

The chain of four definitions is a little complicated, but its function is mainly *heuristic*: it is to lead up to a system of relative probabilities that represents a very powerful generalisation of the customary systems.

In preparation for my definitions, I first introduce, informally, the idea of an *atomic element*, or *atom*, of S , corresponding to an *atomic sentence* (and interpretable by it). It may be based in its turn upon the idea of *structural equivalence* of two elements x and y , symbolised here by $x = y$, and to be distinguished from anything like Boolean equivalence: ' $ab = ba$ ' will be, in general, false (except if $a = b$); and both ' $a = aa$ ' and ' $a = \bar{a}$ ' will *always* be false. Thus $x = y$ holds only if x and y are composed by the same operations of the same elements in the same order. We may then say: if x is in S , then x is a *compound element* of S if and only if there are in S elements y and z such that either $x = yz$ or $x = \bar{y}$; otherwise x is an *atom* of S . Now we can define:

Definition 1. A system S is called *finite* if and only if (i) there exists a finite set A of real numbers such that for every element x of S , $p(x)$ is in A ; (ii) if x_1, \dots, x_n are different atoms, or complements of different atoms, of S , then $p((\dots (x_1 x_2) \dots x_n)) \neq 0$. (Consequently, if x is an atom, then $1 \neq p(x) \neq 0$.)

Definition 2. Let S be finite, and let x and y be in S . Then we define:

If $p(y) = 0$, then $p(x, y) = 1$;

If $p(y) \neq 0$, then $p(x, y) = p(xy)/p(y)$.

Definition 3. Let there be for every positive integer n a finite system S_n such that (a) S_0 is the empty system, (b) every element u of S_{n-1} is an element of S_n (so that, for example, if for every i , u_i is in S_i , then w_n is in S_n where w_n is defined as $w_{n-1}u_n$; \bar{w}_n is, of course, also in S_n); (c) if $p(u) = r$ is true of u as an element of S_{n-1} , then the same is true of the same u as an element of S_n (or in other words, $p_{n-1}(u) = p_n(u)$, so that the indices after the letters ' p ' may be omitted). Then we call the S_n an (infinite) increasing sequence of systems.

¹ Cf. Carnap, op. cit. p. 286

TWO AUTONOMOUS AXIOM SYSTEMS

Definition 4. Let S and T be systems such that (a) every element of any system S_i or T_i (belonging to the infinite increasing sequences S_n and T_n) is an element of S and of T respectively; (b) $S = T$ if and only if $S_n = T_n$ for almost every n (i.e. for every n with at most a finite number of exceptions).¹ Now let x_n and y_n be the general terms of sequences of elements such that x_i and y_i are in S_i ; and let $x = x_\infty$ and $y = y_\infty$ be in S . Then we define:

$$p(x, y) = \lim (p(x_n y_n) / p(y_n)),$$

provided this limit exists; if it does not exist, $p(x, y)$ is equal to the arithmetical mean of the upper and lower limits (which always exist).

This concludes the definition of relative probability on the basis of our absolute system.

3 Consequences for the Theory of Confirmation or 'Rational Belief'

I shall mention here only one important consequence of our system of axioms and definitions: x is inconsistent if and only if $p(\bar{x}, x) = 1$.

The point is important in connection with the theory of confirmation; for it allows us to define $E(x, y)$, $E(x, y, z)$, $C(x, y)$ and $C(x, y, z)$ without demanding the consistency of x .² For having defined relative probabilities so that $P(\bar{y}, y) = 1$ for inconsistent y and otherwise $= 0$, we can replace the definitions (9.1) and (10.2) of the note referred to³ by the following:

Let $P(y) \neq 0$; then we define

$$E(x, y) = \frac{P(y, x) - P(\bar{x}, x) - P(y)}{P(y, x) - P(\bar{x}, x) + P(y)} \quad (9.1^+)$$

Let $P(y, z) \neq 0$; then we define

$$E(x, y, z) = \frac{P(y, xz) - P(\bar{x}\bar{z}, xz) - P(y, z)}{P(y, xz) - P(\bar{x}\bar{z}, xz) + P(y, z)} \quad (10.2^+)$$

¹ Condition (b) is crucially important: if for example we define S merely as the sum of all the S_n , then we can obtain, through re-ordering the sequence, contradictory probability values for the same elements.

My construction resembles in some points that of Carnap's L_∞ ; but apart from its greater generality, it deviates in a number of important points, one of which is condition (b). Another is that I do not operate with individuals; nor is the ' n ' in ' S_n ' related in any way to the absolute number of different atoms in S_n .

² See my note 'Degree of Confirmation', this *Journal*, 1954, 5, n. 1 on p. 147

³ I use, as in the note 'Degree of Confirmation', ' $P(x)$ ' and ' $P(x, y)$ ' instead of ' $p(x)$ ' and ' $p(x, y)$ ' where a certain interpretation (here the 'logical interpretation') of the formal system is intended.

If x and xz are consistent, then these definitions are equivalent to the two older definitions. If not, the explanatory power will be minimal, i.e. equal to -1 .

On the basis of these definitions, $C(x, y)$ and $C(x, y, z)$ can be defined, either by using the newly defined E but otherwise proceeding as in the note referred to, or alternatively¹ by inserting in the denominators of (9.1⁺) and (10.2⁺), respectively, ' $-P(xy)$ ' and ' $-P(xy, z) + P(\bar{z}, z)$ ', which leads to simple C -functions.

The important function is the relativised $C(x, y, z)$. It may be interpreted in this way: divide the evidence e into two parts, y and z , in such a way that $P(y, z)$ becomes a minimum while $P(y, xz)$ becomes a maximum. (This is achieved, largely, by including in z all known initial conditions which together with x allow us to deduce y .) This gives us the best and fairest value for $C(x, y, z)$.

4 A Formal System of Relative Probabilities

Postulate 1. If x and y are elements of the system S , then $p(x, y)$ is a real number.

Postulate 2. If x, y, z and w are elements of S , then xy and \bar{x} are elements of S , and the following axioms hold:

- | | | |
|-----|---|--------------------------------------|
| A.1 | $p(xy, z) \geq p(yx, z)$ | (Commutation) |
| B.1 | $p(x, z) \geq p(xy, z)$ | (Monotony) |
| B.2 | $p(\bar{z}, z) + p(x, z) = p(xy, z) + p(x\bar{y}, z)$ | (Complement) |
| B.3 | $p(xy, z) = p(x, yz)p(y, z)$ | (Multiplication) |
| C.1 | $p(x, xy) = p(y, zy)$ | (Reflexivity) |
| C.2 | If $p(\bar{z}, z) \neq 0$ then $p(x, y) = p(x, y\bar{z})$ | (Independence) |
| D.1 | If $p(\bar{y}, y) \neq 0$ then $p(x) = p(x, y)$ | (Definition of Absolute Probability) |
| E.1 | $(Ex)(Ey)p(\bar{x}, x) \neq p(\bar{y}, y)$ | (Existence) |

From this new system,² the absolute system is derivable. The new system is much stronger than the customary systems in which, for example,

¹ Cf. the last paragraph of my note, 'Degree of Confirmation', this *Journal*, 1954, 5, 149, and my brief communication on the same subject in this *Journal*, 1955, 5, 334

² From C.1 we obtain by substitution ' $p(x, x\bar{y}) = p(\bar{y}, y\bar{y})$ ' which can replace it although it is much weaker; and even its further weakened consequence

$$C.1^- \quad \text{If } p(\bar{y}, y) \neq 0 \text{ then } p(x, x\bar{y}) = p(\bar{y}, y\bar{y})$$

can replace C.1. In C.1⁻, the last ' \bar{y} ' may be omitted, by force of C.2, without loss of independence to either axiom.

TWO AUTONOMOUS AXIOM SYSTEMS

only 'if $p(x) \neq 0$ then $p(x, x) = 1$ ' may be deduced, *but not, unconditionally*, ' $p(x, x) = 1$ '; yet it is still not quite so strong as one might wish. For it allows us to deduce, for example,

$$\text{if } p(uw) \neq 0 \text{ then } p(z, uw) = p(z, wu)$$

only in this conditional form. But we should wish that, whenever x and y are equivalent (in the Boolean sense), then

$$p(z, x) = p(z, y)$$

should be demonstrable *unconditionally*.

In order to achieve this, two ways are open. One is to demand that, whenever the equations ' $p(x, z) = p(y, z)$ ' and ' $p(z, x) = p(z, y)$ ' are both *demonstrable under the condition that the absolute probabilities of the second argument* (i.e. the elements after the comma) *are not zero*, then these equations should hold *without the condition*. This *rule of inference* is easy to handle, but somewhat *ad hoc*. I therefore prefer to introduce in its place a third postulate. (As formulated here, it is stronger than necessary since it would be strong enough if preceded by a restrictive condition like

$$\text{'if } p(x) = p(y) = p(\bar{x}, x) = p(\bar{y}, y) = 0, \text{ then } \dots \text{'})$$

Postulate 3. (Extension to Zero Cases.)

$$\text{If } ((u) p(x, u) = p(y, u)) \text{ then } p(w, x) = p(w, y).$$

This concludes the system. As stated, the absolute system is derivable from the relative system; and *on the assumption* that the S of the relative system satisfies definition 4, all postulates and axioms of the relative system can be derived from the absolute system. But the two systems are not equivalent. The most important aspect of the relative system is that it is compatible with some further demands which contradict the definitions 1 to 4; for example, with the conjunction of the following two demands: (i) if x is an element of S then $p(x) = 0$ or $p(x) = 1$; (ii) there is an element x in S such that $p(x) = 0 = p(\bar{x}, x)$. This shows that the concept of relative probability as defined on the basis of the absolute system, although much wider than the customary concept, is still *essentially narrower* than the concept introduced by the relative system. The most important consequence of this is that, *for purposes of interpretation*, the distinction between atoms and compounds may be ignored. For example, atomic and universal statements may be treated completely on a par—simply as elements, no matter whether atomic or compound.

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A Note on 'The Age of the Universe'

THIS subject, which was discussed recently in a special number of this *Journal* (1954, 5, No. 19) calls for further comment.

Cosmologists claim that the age of the universe is such and such ; and they base their claim on certain observations, mainly, of the recession of extra-galactic nebulae and of the He-content of meteorites.

At first sight this is convincing. We understand intuitively what the statement 'The universe is so many years old' means. Everything has an age ; we know this from ordinary experience, e.g. of the human lifespan. Most of us are agreed also that the statement is metaphorical ; to speak of the age of the universe is not the same thing as to speak of, say, the age of Westminster Abbey ; but the metaphor seems natural and therefore harmless.

The phrase 'the age of . . .' is certainly harmless when referring to ordinary things. But how does it work when applied to 'the universe'?

There are three points I want to raise ; the first is a point of logic. We would normally say that 'universe' designates all there is ; it represents a totality and, moreover, the most inclusive totality that can be imagined. If this is so, how can we ascribe an age to the universe, and what does it imply?

It is well known from the history of philosophy that the grammar of the word 'universe' is peculiar. Whatever has an age must have had a beginning. We can speak of the beginning of the earth and so determine its age ; we mean the date at which a large lump of matter flew off from the sun, etc. The earth was created from something that existed before the act of creation. Similarly, we can speak of the birth of the sun, i.e. the explosion of a supernova, from other matter already existing in the Milky Way. We can speak of our galaxy being created since there were other galaxies already in existence with respect to which we can ascribe a date to the act of creation. But what can we say about the creation or beginning of the universe if there is—by definition—nothing that existed before or outside the universe? 'Creation', 'origin', 'age', etc. have a meaning only when there exists something in space-time that may serve as reference-point. The grammar of the word 'universe' precludes us from speaking meaningfully of its age unless we postulate something that existed before its beginning. If 'universe' denotes all there is, then it is a logical (or semantic) contradiction to speak of its age—unless, whatever exists before is not natural and so is not included in the universe. If 'universe' is a name for everything there is, and if everything *in* the universe has an age, it does

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not follow that we may give an age *to* the universe. This is a logical fallacy, i.e. the confusion between a class and its members.

My second point is about the concept of time. If the universe has a beginning, it would imply that time has a beginning or zero-point. This is the old fallacy of reifying time. But time is not a thing or a process. As we understand 'time' in ordinary discourse as well as in physics, a zero-point *of* time—in contrast to a zero-point *in* time—produces a contradiction. For we can always ask, as we cannot ask in the case of temperature, for example, 'What happened *before*?'

My third point is this. If we accept the ordinary methods of science, then we have first to know the meaning of a hypothesis, i.e. what sort of evidence would make it true, before we can go and look for the evidence. We must know first what we intend to measure before we can determine a numerical value. Here we have the problem that we cannot state clearly what is meant by 'the age of the universe'. How can we say then that this or that is the evidence for it?

The naïve view that we read off our hypotheses from the evidence is no longer defensible today. We understand that data are not simply given; that facts do not speak—only the scientist does. It depends on the interpretation, i.e. on the theory in terms of which both the hypothesis and the evidence are formulated, whether or not the evidence fits the hypothesis. Take the case of the flying saucers as example. We feel it to be very improbable that the earth should be the only planet inhabited by living beings; and there is no doubt that strange shapes and lights have been observed. What is in doubt is whether the evidence relates to the hypothesis. However, we accept the hypothesis that there is life on other stars; we believe in it on grounds that have nothing to do with the specific evidence offered. This leads people to tailor the evidence to fit the hypothesis.

Everyone knows the fallacy of the *ad hoc* hypothesis. Here, as well as when speaking about the age of the universe, we have what might be called the fallacy of the *ad hoc* evidence. The evidence is not false, presumably; there are certain data; nor is the hypothesis invented in order to cover the facts. It is the evidence that is invented, namely, interpreted in a special way, to make it fit a hypothesis which is accepted on other grounds.

But are the data relevant to the hypothesis, and, if so, how far do they support it? This question is crucial because the evidence says nothing about the universe as a whole. The recession of extra-galactic nebulae—if, indeed, it is a recession—is observed only for certain very large though finite regions. And the meteorites come from our own solar system or, at best, from the Milky Way, not even from a neighbouring galaxy. The number obtained from these data may serve for ascribing an age to our solar system or to our galaxy, but not to the universe as a whole.

But can we not extrapolate our data? We always do so, after all, when we take our hypotheses to hold for regions in space-time other than those from which the data were gathered. There is, however, a limit to extrapolation. When we accept a hypothesis as confirmed, we expect it to hold for the *next* occurrence of the event described by the hypothesis. If it is to hold for an *infinity* of instances, then we call it a law or, more likely, a principle, e.g. the principle of conservation of energy. It means that we use the sentence as a rule of designation for the key concept which occurs in it, e.g. for 'energy' (within the theory of thermodynamics). Infinity is a mathematical and not a physical concept; it cannot carry an interpretation that is accessible to observation and experiment. A hypothesis about the whole universe is, however, concerned with infinity, even if the universe is taken as finite in space though unbounded; for new parts of it come continuously into view.

Cosmologists try to counter this objection by saying that we must not separate the physics from the mathematics in our theories, or the logical, formal, meaning of our statements from their extra-logical, non-formal, meaning that can be confirmed by experiment. This argument seems to rest on some kind of fundamentalism, namely, the well-known belief that nature is, at bottom, mathematical. The cosmologists therefore claim that, they make a physical hypothesis when they extrapolate from the finite observed region to the whole universe. Such an extrapolation must result in a tautology, since it indicates that the cosmologists stick to the hypothesis no matter what may turn up; and, fortunately, they speak then of the cosmological principle (perfect or otherwise). It is indeed a principle rather than a hypothesis: no future observations can ever invalidate it and no specific predictions are made. Though with the help of the principle, as with the help of other tautologies, e.g. of logic, we can formulate specific hypotheses, this does not turn it into a statement of fact, as is sometimes alleged. For the principle functions as a guide for theory construction and prescribes the model of the universe which is to be considered. At best, therefore, the cosmological principle is—like the 'law' of uniformity of nature—a tautology; at worst, it may be a myth; but it cannot be an empirical hypothesis.

The points I have raised come from our common understanding of words like 'universe', 'age', etc., and of scientific method. It is conceivable that cosmologists want to propose a different way of looking at them; but then they must explain it. They cannot rely on our ordinary use of words and, at the same time, deny the *semantic obligation* this use entails.

If cosmology is restricted to the description and explanation of the large-scale features of the universe, then there is no difficulty. If it is turned into *cosmogony*, then the theory becomes, literally, meta-physical: its sentences have no meaning within the context of physics, though—as much of meta-

physics—they may have a meaning within another context, namely, of psychology. This is possibly the reason why cosmology is so fascinating to so many people.

E. H. HUTTEN

Seeing and Hearing ¹

SEEING and hearing are quite different phenomenologically. The distinction between the phenomenological character of seeing and hearing has often been drawn, and Broad in a recent article gives a summary of it which may be rendered in the following terms: *Seeing is saltatory and prehensive of surfaces which are localised, while hearing is prehensive of events, but is not saltatory or localised.*² For example, when we say that we see a penny, we generally explain this by saying that we are seeing a limited region which is the surface of the penny. On the other hand, when we say that we hear a bell, we generally explain this by saying that we hear sounds coming *from* the bell; we do not explain it by saying that we are hearing a limited region which is the surface of the bell.

It may be instructive, therefore, to note that the different phenomenological character we ascribe to seeing and hearing derive from just a few differences—for the most part not very drastic—in their physical and physiological conditions. In fact, it is not at all difficult to imagine conditions under which we would feel it natural to say that we heard the surfaces of objects. These conditions will be treated under the following headings: Emitters, Reflectors, Wave-length and Object Size, and Physiological Conditions.

Emitters. An object can be seen under two general conditions; when it emits and when it reflects light. The emitted light may be either monochromatic or it may be made up of a mixture of frequencies. Ordinarily, the frequency distribution of the emitted light is a function of the temperature of the object, and therefore also a function of the states of excitation of its molecular constituents. It is not a function of the mechanical properties of the object as a whole.

In general, an object can be heard only when it emits sound. While the sound may be of a single frequency or of a mixture of frequencies, the

¹ An extended version of this Note was given at the 27th annual meeting of the Pacific Division of the American Philosophical Association, Stanford University, December, 1953.

² C. D. Broad, 'Some Elementary Reflexions on Sense-Perception', *Philosophy*, 1952, 27, 5

emitted sound is a function of the size, shape, and weight of the object ; that is, of the mechanical properties of the object as a whole.

Reflectors. The colour of an object is determined by the colour of the light which it reflects, rather than by the colour of the light which it can be made to emit. Now reflection may take two forms ; regularly, as in the case of a mirror, or diffusely, as in the case of coloured objects. In the case of the mirror, the light is simply bent backward away from the object, but is undisturbed in any other way. In the case of the coloured object, the light may be said to be absorbed by the object and re-radiated in a random fashion.¹ Thus when we see light reflected in a mirror we do not say that we see the mirror, but rather the object which is mirrored ; while when we see light reflected from a coloured object we say that we see the object. In order to distinguish between these two senses of 'reflect' I shall say that objects like mirrors *mirror* light, while coloured objects *reflect* light.

The colour of an object is determined by the colour of the light it reflects when it is observed by the light of a constant emitter of a standard mixture of frequencies, such as the sun. If an object is examined in white light and reflects none, it is black ; if it reflects all frequencies equally, it is white ; and if it does not reflect all frequencies equally, it is coloured.

In the case of sound there is no reflection (in the sense specified above) but only mirroring. When we hear sound 'reflected' from an object, as in the case of an echo, the situation is analogous to mirroring in the case of light. Now the fact that objects do not reflect sound is, I think, of major significance in accounting for the different phenomenological character of seeing and hearing. An object whose surface property is to be perceived must be able to reflect its corresponding signal ; and this in turn requires the object to be so constituted that its elementary parts can be made to assume natural states of excitation which, when superimposed, are responsible for reflection.

It is, however, a purely contingent fact that sounds are not reflected, for we can readily imagine the two conditions under which this would occur. First, corresponding to the sun in the visual sphere, there would have to be a powerful emitter of sounds of all audible frequencies. A battery of amplifiers tuned to these frequencies would serve the purpose. Secondly, physical objects would have to be composed of minute vibrators, each having a natural frequency of vibration. (The object can be imagined to consist of an aggregate of very small springs.) The sound quality of such an object would be determined by the frequencies of the sounds which

¹ This distinction can also be made in terms of the *structures* of the impinging and reflected rays of light. In the case of mirroring, the structures of the impinging and reflected rays are the same, while in the case of reflections from coloured objects the structures of the impinging and reflected rays are different.

SEEING AND HEARING

it reflects when observed under the influence of the standard emitter of sounds. Sound qualities corresponding to white, black and coloured would be attributed to objects in a manner entirely analogous to the case of light.

Under the circumstances indicated, the sound quality of an object would be a surface property like colour, although without the conditions described below it would not be perceived as such.

Wave-length and Object Size. Light waves are very short in comparison to the sizes of ordinary objects, and this accounts for the fact that the objects are seen distinctly. Objects of dimensions of the order of magnitude of the wave-lengths of light do not reflect light in either of the two ways indicated ; instead they *scatter* the light.

The wave-lengths of sound waves, on the other hand, are of an order of magnitude comparable to the sizes of objects, and therefore even under the conditions described above, these objects would scatter rather than reflect the impinging sound waves. In order to have objects reflect sound in the manner in which they reflect light, it would be necessary for them to be of an order of magnitude larger than they are, or it would be necessary to have the sound waves be of an order of magnitude smaller than they are.

Physiological Conditions. When light strikes the eye it is refracted so that rays coming from different directions will impinge upon different areas of the retina. Thus rays from different positions in the field of view cause separate signals to be sent to the optical centres of the brain, and visually extended colour sensations result.

The physiological conditions of hearing differ from those of seeing in that the ear does not contain a lens or a surface sensitive to sound in a manner corresponding to the lens and retina of the eye. However, this is again a purely contingent fact, and it is quite possible to imagine the ear to be equipped with a lens which refracts sound and to have a surface which is sensitive to sounds in the same manner in which the retina is sensitive to light. Furthermore, we can imagine the auditory centres of the brain to be structurally similar to the optic centres.

The foregoing considerations show how the physical and physiological conditions of hearing could be made similar to those of seeing. If these conditions obtained, it is very likely that we would feel it natural to say that we heard the surfaces of objects, just as we now say that we see the surfaces of objects.

W. C. CLEMENT

English and Ontology

DR C. LEJEWski refers in his article on 'Logic and Existence'¹ to the difficulty which English speaking readers have in working intuitively with Leśniewski's singular-inclusion operator ' ϵ '. The source of this difficulty seems to be the fact that in English the two kinds of word which are covered by Dr Lejewski's phrase 'noun-expression' are very different in their syntax. On the one hand there are proper names like 'Socrates', 'Pegasus', etc., and on the other hand there are common nouns like 'man', 'sun', 'dragon', etc. To form a statement out of a proper name or names we use a simple verb (transitive or intransitive) or an expression equivalent to this. Thus we form the statement 'Socrates smokes' out of the name 'Socrates' by means of the verb '— smokes'; we form the statement 'Plato admires Socrates' out of the names 'Plato' and 'Socrates' by means of the verb '— admires —'; and as a special case we have the verb '— is —', meaning '— is identical with —', as in 'Tully is Cicero'. But the operators which form statements out of common nouns are not simple verbs but expressions like 'Every — is a —', 'The — is a —', 'There is at most one —'. And different again is our operator '— is a —', which takes a proper name for its first argument and a common noun for its second (as in 'Socrates is a man'). What consequently puzzles us about ontology is that its variables stand indifferently for proper and common nouns, and we have no operators in our language (except perhaps '— exists', which gives us so much trouble) which are thus indifferent as to whether they have proper or common nouns for arguments. The same difficulty seems to exist in French, though it is much less felt in Latin.

Most English and French expositions of ontology hitherto have attempted to solve this difficulty by translating Leśniewski's ' ϵ ' either as '— is —' or as '— is a —', and then where necessary forcing the English operator to take 'unnatural' arguments. It seems to me, however, that the odd-sounding cases would be far fewer if we translated ' ϵ ' by 'The — is a —'. Dr Lejewski has very ingeniously by-passed the problem by taking as his primitive operator not ' ϵ ' but ' \mathbf{C} ', but in principle his solution is the same as mine. For ' \mathbf{C} ' translates as 'Every — is a —', which resembles 'The — is a —' in that the arguments which it 'naturally' takes (and which Leśniewski's name-variables are therefore taken as primarily standing for) are common nouns (whether these in fact apply to one, less than one, or more than one object); so that we obtain odd-sounding cases only when the blanks are filled by proper names.

This point is not without its relevance to Dr Lejewski's main topic, existence and quantification. In what he calls the 'restricted' interpretation

¹ This *Journal*, 1954, 5, 115

of the particular quantifier, the variables which it is usually thought of as binding are ones standing for proper names ; and proper names are thought of as being somehow directly attached to actual individual objects, though there is some departure from ordinary usage in this, as the case of 'Pegasus' shows. But in what Dr Lejewski calls the 'unrestricted' interpretation, the variables which he takes the quantifier to bind are thought of as standing primarily for common nouns ; and common nouns may of course just as easily apply to no real thing as to one thing or to more than one. So his solution of the 'Pegasus' problem has at least *something* in common with the orthodox solution of replacing the 'name which fails to name' by an expression containing the corresponding predicate (i.e. verb). He doesn't do exactly that ; but he does treat a proper noun as if it were a common one, and this must wear a very similar air in the eyes of anyone accustomed to treating common nouns as logical constructions out of predicates and quantifiers.

This is not, of course, to deny the superiority of the Leśniewskian procedure on the side of formal elegance. It has a further importance too, in that it brings out the fact that the theory of quantification need not be thought of as exclusively a *predicate* calculus. It is rather a quite general operator-and-argument calculus. We can interpret the operators as predicates and the arguments as proper names, but we do not need to do this. We could equally interpret the arguments as statements and the operators as statement-connectives, as in Leśniewski's other discipline of 'protothetic' ; or we could interpret the arguments as common nouns and the operators as statement-forming operators on common nouns, and if with this interpretation we add to the operator-variables an operator-constant (' ϵ ' or ' \mathfrak{C} ') with its own special axiom, we have 'ontology'. With other operator-constants, taking only non-empty common nouns as arguments, and other special axioms, we obtain the 'formalised syllogistic' of Łukasiewicz. From this point of view the most important and illuminating passage in Dr Lejewski's paper is that ¹ in which he urges us to read ' $(\exists x)(Fx)$ ' not as 'There exists an x such that Fx ' but rather as 'the non-committal "for some x , Fx "'. What is important about this proposal is not merely that it removes the suggestion of existence, but that it removes the suggestion that ' F ' must be a predicate and ' x ' a proper name, and it is by removing the latter suggestion that it removes the former.

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¹ Op. cit., p. 113

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DECISIONS AND UNCERTAINTY

I *Introduction*

By the 1870s economists had constructed an elegant explanation of economic evaluations and costs and, consequently, of economic decisions where the outcomes are foreseeable. But many decisions are taken by people who are uncertain what their outcome will be. The outcome of speculators' decisions, for instance, must be uncertain, for if the future could be foreseen it would be discounted and there would be nothing on which to speculate. Again, innovations in the supply of economic goods and services must be based on a hypothesis about the latent demand for them, a hypothesis which can usually be tested only after the innovation has been made. Columbus' decision to sail West was necessarily based on an untested hypothesis. And most pure research is undertaken by scientists who cannot know until it is too late whether their original hunch is fruitless or not. Many wars would not have occurred if the side which was subsequently defeated could have known in advance that this would be the outcome of a decision to fight.

Thus whereas the outcomes of routine decisions can often be predicted fairly accurately, the outcomes of path-breaking decisions which introduce novel elements into the social situation or which significantly influence the course of history are often unpredictable.¹ It might even be claimed that it is only under conditions of uncertainty that we can properly be said to take decisions at all: for if we foresee the outcomes of the alternative courses available to us, and if we know the relative desirability of those outcomes, then our minds are automatically made up. I think this is so except when we see a conflict between our interest and our duty.

While many of the more important decisions have an uncertain outcome they should not therefore be regarded as arbitrary or irrational. Some financiers, explorers, scientists, and politicians have an obvious flair for risky decision-taking: they 'pull it off' pretty consistently, which suggests that their decisions have a *rationale*.

Until recently there has been no satisfactory attempt at an articulation of the *rationale* of risky decision-taking, except in connection with games of chance (and, if we go back to Pascal, in connection with gambles on the

¹ At this stage it is irrelevant whether such outcomes are unpredictable in principle or only in practice. But this question does become important later, and will be discussed in section 4 (iii).

existence of God). This lack of understanding of what is, in two senses, the decisive factor in human activity did not worry psychologists and philosophers unduly. Thus G. E. Moore held that a person ought always to do that action which will promote the maximum good and also that he can never know which action will do this ! The glaring need to find a compromise between these two incompatible assertions inspired Keynes to develop a theory of probability, on the assumption that the incentive of a desirable and certain outcome could be watered down by a numerical probability into the weaker incentive of a desirable but uncertain outcome. But a probabilistic approach to decision-taking under uncertainty has now been strongly criticised in a powerful monograph by Professor Shackle¹ for the very good reason that 'frequency ratios are knowledge and have nothing to do with uncertainty'. In other words, to assign a probability of one-sixth to the chance of my throwing an ace is in practice to say with *certainty* that if I continue throwing the die the proportion of aces thrown will tend towards one-sixth of the total number of throws. And to assign a probability to a possible outcome of a decision is to assert with confidence that if the decision is repeated indefinitely that outcome will tend to recur with this precise frequency. But the decisions of entrepreneurial innovators, with which Shackle is chiefly concerned, cannot be repeated frequently, if at all ; and even if they could, the uncertainty of the innovator's initial hypotheses would render him incapable of knowing beforehand the frequency with which a particular outcome would recur. Thus probability theory is inapplicable to this sort of risky decision-taking. A different approach is necessary.

Since economic theories explain social phenomena by revealing them to be deducible from premisses depicting the dispositions, information, and situations of individuals, major advances in theoretical economics usually consist in defining the form, and in tracing the implications of, some typical human disposition which we recognise when it is described to us though we had previously been unaware of it or of its significance. Shackle's pioneering work is no exception. By replacing numerical probabilities with the subjective notion of potential surprise he has been able to develop a powerful theory of decision-taking in circumstances where neither the outcome of a single decision, nor the frequency with which a particular outcome would recur if the decision were repeated indefinitely, can be known beforehand. Older or less general theories can be subsumed under his as special cases ; it explains what they explain and also phenomena they cannot explain ; and it has important consequences for such divergent branches of economics as trade-cycle theory, taxation policy, and the theory of monopolistic bargaining. Moreover, as a number of the contributors to

¹ G. L. S. Shackle, *Expectation in Economics*, Cambridge, 2nd ed., 1952

a symposium on Shackle's theory ¹ have pointed out, it is a general theory of decision-taking, not restricted to economic decisions in the narrow sense. It is important for all students of men and society.

Before I can discuss it further I must briefly outline the steps by which Shackle constructed his model to depict the procedure of a man who rationally decides on one venture out of a number of alternatives whose outcomes are all uncertain.

2 *An Outline of Shackle's Theory*

(i) Shackle first claims that the agent will *simplify* his problem by focusing attention on two limiting outcomes representing what he 'stands to gain' and what he 'stands to lose'. Professor C. F. Carter regards this as an unjustifiable simplification by Shackle (*U & BD.*, p. 50) but it seems to me to describe a typical mental economy. Where none of the possible outcomes would cause the agent *any* surprise it seems obvious that, as Shackle says, the agent will not focus attention on intermediate outcomes but on the maximum possible gain and on the maximum possible loss. Shackle calls these two extreme possible outcomes the 'primary focus-outcomes'. Usually, however, the possible outcomes of a decision will not all appear equally likely to the agent. How is he to determine the primary focus-outcomes of a venture whose possible outcomes are not only varyingly desirable but varyingly unlikely or surprising as well?

(ii) Shackle assumes that there is one possible outcome between the achievement of which and inaction the agent would be indifferent: this outcome would be neither desirable nor undesirable. He also assumes that this indifferent outcome will lie within an 'inner range' of moderately desirable and undesirable outcomes, none of which would cause the agent any surprise if it resulted from his decision.² On either side of this inner range of unsurprising outcomes lie increasingly desirable and increasingly undesirable outcomes which would increasingly surprise the agent if they occurred. (Thus you might not be at all surprised by anything from, say, a 10 per cent. loss to a 15 per cent. gain on a speculation, but very surprised by a 95 per cent. loss or by a 1,000 per cent. gain.) Shackle assumes that two limits will be reached where the imagined outcomes are so utopian and so disastrous that the agent will regard them as impossible: he would be 'absolutely surprised' if they occurred.

¹ *Uncertainty and Business Decisions*. A Symposium by W. B. Gallie, D. J. O'Connor, I. J. Good, G. P. Meredith, C. F. Carter, B. R. Williams, A. D. Roy, Liverpool, 1954. I shall refer to this book as *U & BD.*

² I think this assumption that an indifferent outcome will cause no surprise should be discarded, since it is not essential to Shackle's theory and since it may be unrealistic—during a boom speculators and businessmen may very well be surprised if a venture does not fetch a profit.

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If we plot the relative desirability or undesirability of the possible outcomes along an x -axis (with $x = 0$ representing the indifferent outcome) and their relative surprisingness along a y -axis (with $y = 0$ representing no surprise and $y = \bar{y}$ representing believed impossibility or absolute surprise) then Shackle's assumptions yield a U-shaped curve whose flat base stands for the inner range of unsurprising outcomes, on either side of which lie increasingly surprising/desirable and increasingly surprising/undesirable outcomes. The upper/outer extremities of the curve represent an impossibly utopian and an impossibly disastrous outcome. There is, of course, no reason why the curve should be symmetrical.

The status of such curves is not immediately apparent and Shackle does not analyse it, so I will try to do so here. Assume that when a decision is taken its outcome at some future date is already precisely determined, although the agent cannot foresee it. Then to an omniscient being all conceivable outcomes of the decision would be 'absolutely surprising' except one, which would cause no surprise. Consequently, from the viewpoint of omniscience, the agent's surprise-curve is necessarily erroneous since it accords equal likelihood to pairs of (often widely) divergent outcomes. It must therefore be regarded as a *subjective* characterisation of a venture's possibilities, shaped by the agent's private hopes and fears rather than by knowledge approximating to the foreknowledge of omniscience. On the other hand, these hopes and fears may be supposed to be governed primarily by past experience. A man who locates the possible outcomes of a venture on a U-shaped surprise-curve is saying, in effect, 'I cannot tell whether this particular venture will prove a *coup* or a damp squib or a dramatic failure or something in between. But experience has taught me that only a few people make or lose fortunes and that the rewards and penalties of speculation and innovation are seldom very big. So I shan't be surprised if I neither gain nor lose much on this venture though I *might* gain or lose a lot.'

(iii) There are various combinations of desirability and surprise which would equally arrest the agent's attention, either as a spur to, or a deterrent from, action. If he has a cautious temperament he will need a large, and if he has a gambling temperament a smaller, increase in desirability to offset an increase in potential surprise. Shackle calls the attention-arresting quality of combinations of desirability and surprise ' ϕ '.¹ For all agents, ϕ will increase as the desirability moves positively or negatively from 0 and as potential surprise moves from \bar{y} to 0. If we construct a three-dimensional contour-map with horizontal x - and y -axes and a vertical ϕ -axis, so that each contour-line represents a constant value of ϕ , then whenever $x = 0$, $\phi = 0$; and whenever $y = \bar{y}$, $\phi = 0$. $\phi = \bar{\phi}$ where

¹ This derivation of a further range of mental intensities from two existing ranges will be critically reconsidered in section 4 (iv).

$x = \pm \bar{x}$ and $y = 0$. To picture this contour-map imagine an east-west coast-line ($y = \bar{y}$) with a chine ($x = 0$) running horizontally and southwards from it. If you walk from the point where the chine meets the coast in a roughly south-west direction you will, as you move further from the coast of absolute surprise and the chine of indifference, ascend a hill of increasingly arresting and bullish combinations of desirability and potential surprise, and, if you walk in a roughly south-east direction, of increasingly bearish combinations of undesirability and potential surprise.

(iv) The surprise-curve described in (ii) represents the agent's characterisation of the possibilities of a particular outcome, and the ϕ -contour-map described in (iii) represents his temperamental attitude towards varying surprising and desirable possibilities in general. The problem was to determine the agent's primary focus-outcomes for a particular venture, and this he can now do by superimposing his surprise-curve for this venture on his quasi-permanent ϕ -contour-map. The surprise-curve attains the highest value of ϕ where it is tangential to a ϕ -contour: here is located that possible outcome whose combination of desirability and potential surprise most effectively focuses the agent's attention. This procedure will yield a pair of primary focus-outcomes for a venture whose possible outcomes are varyingly surprising, corresponding to the best and worst possible outcomes of a venture whose possible outcomes are all equally unsurprising. They epitomise his *maximum reasonable* hope of gain and fear of loss. After repeating the procedure for the alternative ventures the agent has in mind, he will be in a position to assign to each venture its pair of primary focus-outcomes.

(v) The problem is now to decide which venture has the most attractive pair of primary focus-outcomes. But the desirabilities of different focus-outcomes cannot be compared straightaway because they are combined with varying degrees of potential surprise. They have first to be *standardised*, i.e. reduced to terms of sheer desirability unalloyed by any element of surprise. To do this the agent has to find that value of x combined with $y = 0$ which arrests his attention as forcibly as the primary focus-outcome, by following the ϕ -contour on which the primary focus outcome is located round to its intersection with the x -axis and reading off its x -value there. (It is as if the agent asked himself, 'For how much would I sell this uncertain prospect of gain and how much would I pay to be rid of this uncertain prospect of loss?'). The combinations of hope and fear aroused by different ventures have now been rendered comparable with one another.

(vi) The final step in assessing which venture has the most attractive pair of standardised focus-outcomes is to draw what Shackle calls the agent's 'gambler indifference curves' for situations where he stands an equal chance of winning or losing. Let the vertical axis represent gains and the horizontal axis losses. The indifference curve which passes through

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the origin represents those combinations of equally likely gain-or-loss on which the agent would be indifferent between gambling and not gambling. (Normally, it will approximately bisect the angle between the axes and then slope upwards increasingly steeply, since most people become increasingly reluctant to venture a larger proportion of their capital on the toss of a coin.) Each curve above this origin-curve represents a range of combinations of equally likely gain-or-loss which would equally stimulate the agent to 'have a go'. The higher the curve, the stronger will be the stimulus. Now it only remains for the agent to discover which venture has the pair of standardised focus-outcomes which lies on his highest gambler indifference curve and then, if this curve lies above the origin-curve, to set the venture in motion.

3 *Comments*

(i) It is easy to apply Shackle's general theory to decision-taking in special circumstances. (a) A venture, such as a bet, whose possible outcomes are discrete, will have a discontinuous surprise-curve. The two points on it whose ϕ -values are highest will be the primary focus-outcomes. (b) In a genuine probability-situation where we do know beforehand the frequency with which a particular outcome would tend to recur if the decision were repeated indefinitely (as in games of chance) the surprise-curve for the possible outcomes of a single decision would be based on their probabilities (though I am not sure whether Shackle's general hostility to a probabilistic approach to risky decision-taking would allow him to accept this). (c) When an agent's expectations are clarified, when previously unanswered questions about the consequences of a decision are answered, the gap between his focus-outcomes will diminish, vanishing if he comes to regard one outcome as certain.¹

(ii) I now turn to the relation of Shackle's model to actual decision-taking.

It is doubtful whether anyone, even the author of the theory himself, comes to decisions by constructing surprise-curves, ϕ -contours, and gambler

¹ Shackle argues that since the gap between focus-outcomes will be diminished by clarification, a shift of either outwards will involve a much larger, and therefore more surprising, shift by the other than would a shift of either inwards: only some very surprising and unthought-of event will cause his focus-gain or focus-loss to increase significantly. But whereas an unthought-of bearish factor will usually act as an immediate deterrent, Shackle claims that the agent may find it difficult to assimilate an unthought-of bullish factor into his structure of expectations. Shackle concludes, 'This asymmetry seems at least partly to explain why the downturn of investment and employment after a boom is usually more abrupt and rapid than their upturn after a slump' (p. 76).

indifference curves. But this does not mean that the theory is an unrealistic caricature of decision-taking. What we needed was not a phenomenological description of what passes through the mind during the process of deciding but a reconstruction of the *rationale* underlying skilful and risky decision-taking, and this is what Shackle has given us. His theory does not depict introspectible events but a formal set of human dispositions meshing, so to speak, with formal characterisations of alternative ventures. Now people can act according to their dispositions without being able to describe them ; they can unconsciously respond to the unverbaised *feel* of a situation ; and they can act rationally, as they can write prose, without realising that they are doing so. A brilliant young chess-player once told me that his subsequent analyses of those of his matches in which only a few seconds were allowed for each move had revealed only one move which he would not have made if he had had time to calculate its repercussions. The relation between Shackle's theory and actual decisions is somewhat similar to the relation between the chess-player's patient *post-mortems* and his rapid and intuitive moves in a match. His *post-mortems* enabled him to understand (or, should he have forgotten them, to retrodict) his moves, except on the rare occasions when his intuitive decisions were less rational than his deliberate reconstructions. Similarly, Shackle's theory is a patient reconstruction of the formal character of decisions which may be taken rapidly and intuitively. Mr A. D. Roy is wrong to complain that Shackle's theory naïvely assumes that the agent will derive each decision from first principles (*U & BD.*, p. 73).

Roy, who devotes his paper to the problem of testing Shackle's theory, commends the compilation of case-histories by economists attached to firms, in order to see whether counterparts to Shackle's model can be found in the world of commerce. Such studies would no doubt yield much interesting material ; but I think that as a method of testing they would be too direct. A theoretical model of the atom is tested indirectly by looking for consequences, not directly by looking for counterparts in nature. The structure of a businessman's dispositions and latent know-how is just as invisible as the structure of an atom and years of patient peering will not tell you whether that structure and Shackle's model are isomorphic or not. Since less general theories can be subsumed under Shackle's, it can obviously explain what they can ; therefore, to test its claim to superiority one ought to confront it with events which they ought to but cannot explain. Shackle himself gives an example : according to the Uthwatt Committee, the value of a plot of land in a belt surrounding an expanding town is higher than the value obtained by multiplying its potential price as a building-site by the probability of its being selected for this. Carter derives the following prediction from Shackle's theory : if bondholders regard rises in the price of a bond as possible signs of its early redemption, then subsequent

risks should be slow and even on a probabilistic theory of decision-taking, and should be abrupt and large on Shackle's theory. These impress me as good examples of the fairly precise kind of occurrence by which the theory should be tested.

4 Criticisms

(i) I have no strong feelings about Shackle's dispositional analysis of 'degree of belief' in an outcome in terms of the inverse of the amount of surprise the outcome would cause if it occurred, but I cannot accept his supporting argument. He says that 'a degree of belief is not in itself a sensation or an emotion' whereas a feeling of surprise is a 'concrete mental experience' (p. 10). In other words, surprise is the genuine article, and to talk about 'degree of belief' is only a way of talking about potential surprise. Yet some people believe strongly in life after death although no potential surprise attaches to this belief: either the believer will be able to say, in some other existence, 'As I predicted', or he will not be able to experience surprise. Moreover, Shackle himself does not identify 'absolute surprise' with an emotional state (would one faint with absolute surprise?); on the contrary, he derives this notion from the idea of believed impossibility—to talk about 'absolute surprise' is only a way of talking about the highest degree of disbelief. Professor W. B. Gallie proposes defining more or less surprising outcomes as outcomes which conflict with more or less confidently held scientific hypotheses. I think this is on the right lines but I do not think that surprise is necessarily caused by the refutation of anything so articulated as a scientific hypothesis. I should be extremely surprised if I dug up a diamond in my garden, but I should abandon some unformulated belief, rather than a scientific hypothesis.

(ii) Another minor criticism of Shackle's formulation of his theory is that he continually speaks of 'enjoying by anticipation' the imagined outcome of a venture, and he intends the word *enjoying* seriously. He speaks of the pleasure of a future outcome as though it were a cheque which could be repeatedly cashed in advance at a discount which diminishes until the cheque is finally cashed at par when the outcome materialises. This involves him in an infinite regress: 'Since to enjoy by anticipation is itself a pleasurable act, and can itself be imagined in advance, it can give rise to a secondary enjoyment by anticipation, in which we enjoy the prospect of enjoying the prospect of a pleasure-giving event. And there can evidently be a tertiary, etc., enjoyment by anticipation' (pp. 70-1). Shackle regards this as an ingenious discovery, but I think his introduction of all this fictional pleasure does more credit to his heart than his head. 'Don't waste your sympathy on the couple who can't get married for five years—they're thrilling with anticipations of anticipations of. . . . Pity them, rather,

when they've only their honeymoon to enjoy'—that is surely a lop-sided view. A pleasurable event which seems certain to occur may be awaited with impatience; and if it is not certain to occur, with anxiety as well. Of course, some people do enjoy imagined situations; but the more they do so, the more they indulge in day-dreaming, the less incentive they have to take the kind of decision for which Shackle designed his theory. Enjoyment favours the *status quo*.

(iii) One significant criticism which recurs in *U & BD*. is that Shackle makes no attempt to 'objectify' his surprise-curves: they are merely subjective characterisations, based on private hopes and fears, of a venture's possibilities, and do not correspond with objective likelihoods. This complaint tends to be associated with a vestigial reluctance to abandon a probabilistic approach to decision-taking; for when all Shackle's strictures have been taken to heart the fact remains that we can sometimes assign an objective probability to an event whereas surprise is a subjective emotion.

Both Professor D. J. O'Connor and Professor G. P. Meredith try to indicate where one might find a measure of objective credibility on which surprise-curves might be based, but I must say that I do not find their hints very encouraging. Thus Meredith wants surprise-curves to be drawn according to 'the pattern of evidence of rational likelihood' as though that did not beg the question.

I think they have failed to find an objective counterpart to potential surprise for the simple reason that it cannot exist. Using 'evidence' in the widest sense to cover everything relevant to the prediction of an outcome, there are three possibilities: either (a) the total available evidence does determine what the outcome will be; or (b) while not determining any single outcome, it does determine the frequencies with which different outcomes will recur if the action is repeated indefinitely; or (c) the total available evidence is consistent with mutually exclusive forecasts of the outcome, or of the frequencies with which outcomes will recur if the action is repeated. In situation (a) the surprise-curve of an agent who knows less than the total available evidence cannot be 'objectified'; it can only be made to shrink into a 'fore-knowledge point' as described in section 3 (i) (c). To seek an objective counterpart to potential surprise is self-defeating, here: either you succeed and leave no room for surprise or you fail and leave surprise in a subjective state. The probability-situation (b) does allow the surprise-curve for a single event to be based on objective knowledge although it leaves no room for surprise about subsequent frequencies over long runs. In situation (c) the surprise-curve cannot be objectified (not because potential surprise would melt into foreknowledge but) because there can be no foreknowledge of the outcome.

Those critics who demand a procedure for objectifying Shackle's surprise-curves are, in effect, making two claims. First, situation (c) never arises:

the total available evidence always does determine either the outcome or the frequencies of outcomes. Secondly, and arising from the claim that outcomes, or their frequencies, are never in principle unpredictable, Shackle's theory is only a second-best affair : it only tells us how to go about decision-taking if we are content to be uncertain about its outcome. The ideal theory of decision-taking under uncertainty would be self-destructive : it would give us a procedure for transforming uncertainty, if not into certainty, at any rate into probability. The old desire for certainty has become the modern desire for probability and it will not tolerate the reactionary thesis that the outcome of a situation may be neither certain nor probable but in principle uncertain. Thus O'Connor writes : ' I certainly would not agree that we have any reason for believing that there are features of human behaviour, whether individual or social, that are *in principle* unpredictable ' (*U & BD.*, p. 17).

The trouble is that we *do* have reasons for believing this. The hypothetico-deductive interpretation of science, which we owe to Professor K. R. Popper, teaches us that science cannot predict the content of future scientific discoveries. For a scientific advance consists in hitting upon some more universal hypothesis from which, in conjunction with special non-universal assumptions, existing hypotheses may be deduced as theorems, observation-statements being deduced from the hypothesis and theorems in conjunction with descriptions of initial conditions. Each advance leads to hitherto unthought-of observations at those crucial points where the implications of the new and more universal hypothesis extend beyond those of the old. Consequently, a path-breaking hypothesis could never be derived from *existing* observation-statements. Nor could a path-breaking hypothesis (e.g. Newtonian theory) be derived from existing lower-order hypotheses (e.g. Kepler's and Galileo's theories) since it says more than they do and will usually show them to be false unless they are saved by special assumptions. The logical derivation is always from the new hypothesis to new versions of existing hypotheses and to new observation-statements and *not* from existing observation-statements and hypotheses to the new hypothesis. There is thus no possibility of deducing, and so no possibility of rationally predicting, new scientific hypotheses from existing information. And if, without a rational procedure but with luck and genius, you hit upon a new hypothesis you will be too late to predict its discovery.

Now it is a fact that scientific advances occur ; and it is a fact that businessmen sometimes weigh the possibility that the innovation they are considering may be rendered obsolete by a scientific discovery. They then confront a situation whose outcome is in principle unpredictable.

Then there is the situation where predictions about its outcome are an intrinsic part of the situation itself, where, for example, to borrow an

illustration from Keynes, 'we devote our intelligences to anticipating what average opinion expects average opinion to be' (*General Theory*, p. 156). The basis of Popper's famous attack on historicism is the fact that it is typical of human society that predictors are enmeshed in the system they are trying to predict, so that their predictions either cause their own falsification or, if they cause their own verification, must have been illogically derived from the information available when they were being calculated, since the prediction itself was not then a factor in the situation.

Thus there exist situations whose outcome the agent is in principle unable to predict, situations where he is necessarily unable to replace potential surprise by foreknowledge or by knowledge of the probabilities. And even in cases where the total available evidence does determine what the outcomes of alternative ventures would be the agent may soon reach a point where the more time he spends acquiring further data the more out-of-date his existing information becomes. I conclude that Shackle was quite right to assume that some decisions have to be made whose outcome is in principle and/or practice uncertain instead of trying to provide a method which should, *per impossibile*, render their outcomes certain or probable.

(iv) I now turn to a dangerous criticism which Carter has aimed at Shackle's theory and which would sink it if evasive action were impossible.

Between the two natural termini, absolute surprise and no surprise, degrees of potential surprise can only be ranked in a comparative order. One can say, 'This would surprise me more than that' but not 'This would surprise me twice as much as that or by 0.3 of the amount of surprise which something I believe impossible would cause me.' Again, we can only rank degrees of desirability in a comparative order on either side of the natural zero provided by indifference. We cannot say *how much* more pleasure one outcome would give us than another. (Translating desirabilities into monetary terms will not help because the utility of money varies with the amount.)

Now Shackle's attention-arresting quality ϕ is a function of surprise and desirability. And Carter's objection is that it is logically impossible to derive from two merely comparative rankings a further comparative ranking. For if we are to know which of two possibilities, one of which is both more surprising and more desirable than the other, will most attract us we must first know how much more surprising and how much more desirable; and this is precisely what we cannot know. Carter puts it this way. If we do ascribe numerical values to the likelihoods and desirabilities of different possibilities we can give them any values we like provided they are in the right order, and their compound ϕ will vary according to our arbitrary ascriptions. If, having given one possibility a likelihood of 0.8 and a desirability of 10, we give a second possibility 0.5 and 20, the second

is made more arresting. But if we give the second 0.4 and 17—and there is no reason why we should not—then the first will get a higher value of ϕ .

Thus Shackle's y -axis is like an elastic string fixed at both ends, and his x -axis is like a straight elastic string fixed in the middle. Values on these axes will never get out of order but they can be bunched, dispersed, and displaced at will; and values of ϕ will vary accordingly.

What is needed is some way of *tying down* likelihoods and desirabilities so that any pair of them yields a *stable* compound. We cannot assign them non-arbitrary numerical values. Is there another way? Popper has shown me that there is; and when I re-read Carter's paper I realised that he was working towards the same solution. I will give the solution in my own words, but I am not its originator.

The problem may be approached by way of an analogy. Suppose that someone is selecting a cloth from samples which are of various shades of grey and of various textures. He prefers paler to darker shades and softer to coarser textures. If we ask him whether he would prefer another cloth which is both paler and coarser than the sample before him, he will ask, 'How much paler and how much coarser?' and we will not be able to answer so long as the shades of grey and degrees of texture are treated purely comparatively. But now suppose that he can identify and name ten separate shades of grey of decreasing desirability (e.g. white, silver, pearly, ash-grey, slate, battleship, charcoal, etc., black) and ten separate textures of decreasing desirability (e.g. silky, downy, velvety, shaggy, bristly, etc.). He will then be able to identify one hundred separate colour-and-texture combinations; and since each combination is unique he will be able to rank them on a desirability-scale. (He may be indifferent between some combinations.) If we now ask him whether he would prefer a slate-coloured velvet, say, to a charcoal-coloured silk, he will be able to answer.

Now substitute for shades of grey separate and identifiable degrees of surprise (e.g. flabbergasted, startled, surprised, mildly disconcerted, not at all put out, etc.) and for textures substitute separate and identifiable degrees of pleasure (e.g. ecstatic, delighted, contented, indifferent, disappointed, dejected, miserable; alternatively, identifiable states of pleasure might be *named* by sums of money). The elastic axes have now been pinned down: each pin represents a discrete state of pleasure or surprise, any pair of which provide a stable and therefore comparable attention-focusing combination. The loss of elegance involved in substituting discrete points for the smooth curves for which Shackle has an economist's partiality is off-set not only by the removal of a fatal flaw in the derivation of attention-arresting qualities from merely comparative degrees of surprise and desirability, but also by greater realism in depicting surprise and desirability themselves. It is misleading to represent the mind as if it were a system of thermometers capable of registering continuous ranges when in fact it registers only crude qualities.

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5 *Summary*

Although there are ineradicable elements of uncertainty in human life, and although it is the most significant decisions whose outcomes tend to be the most uncertain, the classical theory of decision-taking, common to both the philosophical and the economic utilitarians of the nineteenth century, presupposed foreknowledge of the decision's outcome. In the twentieth century foreknowledge was reduced to knowledge of the probabilities of the possible outcomes of a decision, in line with a general tendency to substitute probability for certainty. However, it is only in special and untypical situations that a probabilistic approach to decision-taking is applicable. Where the agent faces irreducible uncertainty about the outcome of a decision (or about the probabilities of its possible outcomes) he has to fall back on a subjective characterisation of the decision's possibilities (though this subjective characterisation will be largely shaped by his general experience of life's rewards and penalties). Starting with the subjective notion of potential surprise, Shackle has developed a general theory of decision-taking which embraces narrower theories and which can explain some facts which they cannot. It formalises the *rationale* of those skilful decisions, taken more or less intuitively, which would have been the same if they had been taken after careful calculation and with full self-knowledge. As it stands it has a serious flaw: it pretends to derive one comparative order of mental intensities by combining intensities from two other merely comparative orders, although this is impossible. However, between continuous ordinal ranges of qualities and continuous cardinal ranges of quantities there lies a third possibility: discontinuous ranges of separately identifiable qualities. And if such discontinuous ranges are inserted in place of Shackle's ordinal ranges of surprise and desirability his theory, which was under sentence of death, is, I am thankful to say, granted a full reprieve.

J. W. N. WATKINS

THE METHODOLOGY OF PSYCHO-ANALYSIS

THIS monograph¹ is concerned with methodological problems arising out of psycho-analytic theory. The author begins with the sobering admission

¹ *Psychoanalysis and the Unity of Science*. By Else Frenkel-Brunswik, in *Contributions to the Analysis and Synthesis of Knowledge*. Proceedings of the American Academy of Arts and Sciences, 1954, 80, No. 4, 271-350. \$1.55. Published in co-operation with the Institute for the Unity of Science.

that the theory is in need of a more precise formulation if we want to establish clearly the relation between hypothesis and fact in this domain. Mrs Brunswik therefore examines first how operationalism (or operationism as it is nowadays called in the United States) can help to achieve this aim ; though somewhat sympathetic towards it, she sees that explanations of this sort are severely limited. Even in modern physics—where, after all, operationalism originated—we allow more freedom for our explanations than the operationalist would seem to grant. We no longer require that every statement, not even every factual statement, must be directly testable by a specific operation. Indeed, this ideal was never really justified and is obviously impossible to attain. We can of course water down the operational analysis and regard writing as a paper-and-pencil operation and thinking as sub-vocal speech ; this makes operationalism more comprehensive but useless, since it is applicable only by means of verbal tricks. In a modern scientific theory only a few statements need be anchored to experiment, provided of course that the theory as a whole is reasonably well constructed, i.e. that its statements hang together logically. Psycho-analytic theory is seen here from this, more liberal, point of view.

It is the assumption of unconscious processes that, according to Freud, characterises psycho-analysis as a natural science. He expressed it by saying, in *An Outline of Psychoanalysis*, that ' it is possible to establish the laws which those processes obey . . . '. Here, I think, we see the difference between the scientific concept of the unconscious and the philosophic concept that had been current for some time before Freud. It is the same difference that exists between, say, the Greek concept of atom and the concept used in modern physics. The idea alone, important as it is, does not suffice : we must be able to find definite processes and specific mechanisms which can be described by its help. This makes the idea fertile ; and it was exactly what Freud did when he described in detail the various unconscious processes that he had discovered in clinical practice.

The term 'unconscious' is then introduced by giving, explicitly, its rules of use ; and so laws are formulated that can be instantiated. Mrs Brunswik is inclined to take the term as dispositional though she also speaks of it as 'an abstract, hypothetical construct'. It is clear, however, that Freud was aware of the danger of reifying the concept and did not want to invent 'another man inside us'. Though speaking at first of the various systems, i.e. unconscious, preconscious, and conscious-perceptive, he never intended this 'topography' to serve for localising mental processes within the human body. Obviously dreams and phantasies cannot be spoken about in this manner, for the unconscious is not describable in terms of ordinary space and time. Instead Freud speaks of mental energy and developed what he called the 'economic point of view'. By making use of Helmholtz's principle of conservation of energy it is possible to mitigate,

if not to avoid completely, the usual space-time description. This is, in my view, very similar to what was done in modern physics ; for the concepts of energy, state, and system allow us to get away from the standard description in terms of things and to introduce a language that is more suitable for describing a process. We are perhaps still very far from having a proper process-language ; but in psychology a thing-language corresponding to Newtonian mechanics, with its billiard-ball universe, is unacceptable. The energy-language allowed Freud to develop a theory of unconscious processes which is logically simple, since it is based upon a single concept. Of course, many difficulties and dangers remain : one can misuse the term 'unconscious process' as one can misuse 'unconscious', e.g. by forgetting that a process is something dynamical. Perhaps the safest thing is to speak always of persons being unconscious of certain mental processes, as J. O. Wisdom remarked some time ago, instead of using 'unconscious' *tout court*.

The concept of instinct, 'conventional but still rather obscure' as Freud called it, is next discussed in the monograph. Mrs Brunswik points out that the biological processes underlying instinctual manifestations are not fully known, but this does not mean that the concept is empty or that the instinct theory is useless. It would have been so only if Freud had invented new instincts to fit every sort of behaviour ; his classification of instincts, however, delimits the domain in which the concept is applicable. The most doubtful assumption is of course that of the death instinct, which is rejected today by many analysts as unnecessary. Mrs Brunswik feels here that we should postpone judgment ; and Freud himself had argued that much will depend on the results which future research in biology might produce. The analogy to the concept of entropy—sometimes put forward in this connexion—is, in my opinion, dangerous and might mislead us into circular reasoning. For, in fact, 'entropy' is often misinterpreted in terms of death, e.g. when people speak about the heat death of the universe which is supposed to follow from the second law of thermodynamics. Still, one cannot help being struck by a certain analogy. The unconscious is a (practically) closed system ; it is nearly as timeless as the closed thermodynamical system ; and if one starts with the idea of energy conservation, clearly a principle of 'waste' is needed if one wants to describe a genuine dynamical process.

The objections that have been raised against the concept of instinct are quite understandable. Too often philosophers and scientists alike have misused it by ascribing very specific 'innate' faculties to human beings. Moreover, instincts seem to undergo so many, and strange, transformations—especially that of sex which is more 'compressible' than any other instinct. But the concept is of value, both for description and for explanation if, with Freud, instincts are regarded as a kind of energy subject to the conservation principle ; it allows us to introduce some sort of quantitative measure for them. The difficulty is, as I see it, that we are plagued by

dubious dichotomies such as nature versus nurture, or innate ability versus learning, which have at best only a limited applicability. After all, no one has to learn how to eat or to make love—even though learning plays a rôle in the civilised manifestations of these instincts. And we can specify the mechanisms, e.g. of repression, or of sublimation, etc., which produce the behaviour pattern of civilised man. Certainly, the American neo-Freudians have gone too far in emphasising the influence of the social environment. I am tempted to say here that they are literally casting out the baby with the bathwater; for the, as yet uncivilised, baby has definite urges, and it is these we want to describe by means of 'instinct'. It is amusing to note, *en passant*, that this Lamarckism flourishes mainly in the 'young' countries, i.e. the United States and Russia, for obvious social reasons. There are of course other motives for accepting such an attitude in psychology, for example, that by putting the blame for our sins on the environment we can assuage our feelings of guilt; and this makes the doctrine of the American school more comfortable than the original theory.

Freud already had characterised the instincts as 'mythical beings, superb in their indefiniteness'. But he argued that we should accept the term all the same since it is no more obscure than was, for instance, the concept of force in the beginning of physics. It is not true 'that science should be built up on clear and sharply defined basic concepts. In actual fact no science, not even the most exact, begins with such definitions . . .'. Indeed, what we need is to know the meaning of a term, that is, how to use it in a specific context, and not its definition which presupposes that we know the meaning already. We only slowly work out the meaning, or meanings, of a technical term. In physics, for example, we employ 'electron' in many different ways, according to the theories in which the term occurs; but I do not think there is a single textbook where we could find a definition for it. Hence the objection that the lack of clear-cut definitions for the concepts of unconscious and of instinct puts psycho-analysis beyond what is logically and heuristically justifiable cannot be sustained.

Psycho-analysis, exploring human personality 'in depth', necessarily requires a higher, more theoretical or 'abstract', level of description. Instead of describing human behaviour directly in familiar terms, the theory interprets it by means of a few unfamiliar concepts. This has given rise to various criticisms, namely, (i) that the theory is too far removed from ordinary experience and (ii) that it leads to contradictory explanations.

The first objection about the 'remoteness from the phenotype' (as the author calls it) is, I think, easily met. The same situation is found in physics where we may describe heat phenomena either by macroscopic thermodynamics or by the microscopic kinetic theory. The microscopic description is, in general, more powerful; not because it treats of 'smaller units', e.g. atoms, but because its concepts are logically stronger. 'An advanced

theory is characterised by the fact, among others, that it is based on one, or at least very few, powerful concepts instead of using many logically weak, though familiar, terms. In this way we manage to explain the visible by the invisible, and one invisible by another invisible. It is true that such a theory brings with it increased difficulties in description, interpretation, and confirmation. Mrs Brunswik rightly counts it to be a merit of psycho-analytic theory that it belongs to the more advanced type.

The second objection is more serious and would be fatal to the theory if it were true. It is said that certain overt behaviour, e.g. friendliness, may have to be explained in different, and sometimes opposite, ways at the same time. Thus the friendly handshake of a neighbour may express his hostility against me and even represent a bodily attack on my person.

There are, in my opinion, at least three mistakes in this argument. (a) The first mistake is about the basic concept involved here; for the contradiction of terms, like friend and foe or love and hate, is taken over from ordinary discourse into the psycho-analytic context. There, however, the basic term is ambivalence (just as in mechanics, for instance, a particle may exert an attractive and a repulsive force at the same time): what we assess is the relative strength of the two components. (b) Second, there is a confusion of logic with psychology. Though the logic or, better, the semantics of our words in some way mirrors what we talk about, logic is not concerned with the same sort of things as physics or psychology. Logic is a theory about words, while physics is a theory about certain phenomena and psychology is a theory about some other phenomena. Words, physical things, and mental processes are all in one sense natural phenomena; but contradictions do not occur in nature like electrons or feelings of pain. Contradictions arise from our employment of words. We may say, for example, that he loves and hates her at the same time. But psychological conflicts are not therefore contradictions, though we may use words like love and hate here which normally function as contradictories. (c) Finally, there is a methodological mistake. We have here different kinds of explanation which, when given simultaneously, may appear to be contradictory. When my neighbour shakes hands with me, he may be conscious only of friendly feelings, though I interpret—and perhaps correctly—his action in terms of unconscious hostility. It all depends on which explanation I prefer; both may be equally acceptable, but then I must not confuse the one with the other. In ordinary life it is usually sufficient to explain behaviour in terms of conscious motives and processes; the explanation by means of the unconscious is best left to the analytic session.

Mrs Brunswik then discusses the type of explanation which psycho-analytic theory offers to us. She emphasises that the theory is neither introspectionist nor operationalist; both kinds of explanation are unreliable, for we can 'lie' with our deeds as well as with our phantasies. Still, the

author is inclined to think that the shift 'from overt behaviour to underlying dynamics was too radical in psychoanalysis'. Sublimation is here given as an example of a process for which the conditions cannot be fully described in present-day theory; to explain the transformation of an instinct into socially accepted behaviour requires us to refer to the community in which we live. Though Freud has made an effort to evaluate the environmental influence by considering the ego defences, the author concludes that psycho-analytic theory cannot as yet explain adequately the rational and social behaviour of human beings.

It seems to me that we must be very cautious here. Though neglect of 'external' factors may lead to misinterpretation, it is a fact that the 'internal' factors are often so much stronger; otherwise we could hardly account for the observed stability of human character. Moreover, the external influence, e.g. of parents, is effective mainly through the internal agency of the superego. We have to recognise that the boundary between the inner and the outer world is blurred and changes continually during an individual's development. That is, the genetic viewpoint taken by psycho-analysis does not easily allow us to apply the external/internal dichotomy which, perhaps, fits better into a static theory.

Apart from discussing the kind of explanation psycho-analysis provides, Mrs Brunswik also investigates its logical type. Four levels of scientific discourse are distinguished. The first is the level of straightforward description of our observations; the second is that of empirical laws; the third level generates what is called here a first-order theory, i.e. a theory from which empirical laws can be derived; and the fourth level is that of second-order theories, which are said to be heterogeneous, or combine different kinds of concepts, e.g. of physiology and of (behaviouristically formulated) psychology. Psycho-analysis is said here to belong to the third or, possibly, the fourth level.

This terminology has been current for a long time, but I wonder whether it does not create more problems than it solves. I do not want to deny that we can distinguish different levels in our descriptions; what is doubtful is, in my view, the criterion by which a 'higher' level is distinguished from a 'lower' one and how the levels are related to each other. It involves several dubious assumptions, namely, that we must start with the rockbottom of experience; that there exist special sentences which by their very nature express this experience; and that we can arrange our theories in a purely logical hierarchy ruled by the deducibility relation. This way of speaking is, as I see it, based on an antiquated philosophic doctrine, i.e. of some sort of empiricism. (Moreover, it introduces, needlessly, all kinds of ontological complications, e.g. the mind-body problem.)

But we do not build up our theories from below only, we also build down from above; guided by a hypothesis we go out and search for

evidence. Our theories are not based on a passive submission to experience, as empiricism suggests, but on actively making observations and experiments; what we accept as data depends on the theory for which they are intended as support. And when we take a collection of statements as a theory, in a rather loose sense, it is not by logic alone that we can relate one such theory to another. It is a matter of interpretation; and this depends on the meaning of the principal statements in each theory, on the semantic type of the key-concepts employed, on the models underlying the various theories, and on other extra-logical and extra-linguistic considerations. If such a hierarchy can be constructed at all, it seems to me that only something like a correspondence principle can do it, as in physics. That is, we can relate a higher to a lower theory if we know the factual conditions under which a certain statement in the first theory describes the same event as another statement in the second theory. It is not in the power of formal logic to provide this relation. Mrs Brunswik rightly criticises the excessive emphasis on logic which philosophers so often show when they make unwarranted generalisations or introduce dichotomies *ad libitum*; the conceptions employed here seem to me to suffer from the same defect.

A similar objection may be raised when Mrs Brunswik asks whether 'psychoanalytic concepts are hypothetical constructs or intervening variables'. Hypothetical constructs are supposed to contain assumptions about something not directly observed; while statements involving intervening variables are said to be expressed in terms which relate to experiment, either directly or *via* reduction sentences (i.e. a certain kind of double conditional). I should dispute, for example, the claim implicitly made here that reduction sentences are a logical device that can transfer meaning from observation sentences to theoretical statements or relate expressions of different semantic type. Nor can I believe that variables are, so to speak, provided by nature while other concepts are constructed by man. Indeed, the whole distinction is not very clear to me: as if there were concepts that are not constructed, i.e. invented, and this does not exclude the case in which a conceptual variable designates a variable property, e.g. in the equation of state

$$f(p, V, T) = \text{const.}$$

This discussion leads the author to the theoretical problem of confirmation; and she adopts the view of Carnap and Hempel that our hypotheses are law-like statements involving 'open' dispositional terms which can never be conclusively verified. This introduces all the well-known difficulties of probability inference which cannot be discussed here. There is only one point I should like to mention, namely, that the word 'open' has a different sense when applied to concepts or to theories. A scientific theory certainly contains more than the observations upon which it was originally based; for it enables us to predict future events. Freud's

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hydraulic model of the libido—a rudimentary theory—is an example ; it allows us to speak of an instinctual need as being dammed-up, or as tending to regress to an earlier channel, etc. Though we know that the mind does not contain tubes and pipes, the model shows up the relation between the various factors and so we arrive at new predictions. In other words, the open character of a theory depends on the imagination of the scientist who invented it and how he uses the theory as a tool for research ; but the concept of probability supposed to keep the theory open for new knowledge does not enter here at all. (A different argument, though leading to the same conclusion, applies, I think, to the ‘open’ concepts.)

The discussion then passes to the practical problem of confirming a psycho-analytic hypothesis. Therapeutic practice has certainly provided ample substantiation, and a good deal of statistical material has been accumulated. Freud had pointed out already that the evaluation of data is not as simple in psycho-analysis as it is in physics ; for the ‘personal equation’ of the observer is of greatest import here. It is therefore necessary that the observer has himself been analysed ; and this is sometimes taken by opponents as indicating that psycho-analysis is a closed, esoteric, affair. I do not think one can really say this ; for certainly in a more advanced physical experiment only the trained physicist ‘sees’ what is happening and so can collect the relevant data, while the layman looks on uncomprehendingly.

In contrast to physics, and in common with the ‘living’ sciences, psycho-analysis demands the genetic approach. An aetiology rather than the simple, so-called causal, law is needed. This is bound to make confirmation more difficult, since not only present but also past data and observations must be collected. To obtain knowledge of past events and phantasies depends mainly on the patient’s memory ; and one’s memory is notoriously unreliable, especially in matters of sex, quite apart from the amnesia that hides our childhood experiences. Data relating to the pre-verbal stage of infantile development may also be required, and these, as Mrs Brunswik remarks, ‘offer staggering methodological difficulties’. Nevertheless, the aetiology of a great variety of neurotic, and even psychotic, behaviour has been established ; definite diseases and their symptoms have been identified ; and character types, etc., have been recognised. This has made possible prognosis and prediction for certain individual cases in which the relevant data were not complete.

It is true that ‘predictions about phantasies . . . are markedly superior to predictions based on . . . overt behaviour’. We normally gain access to our phantasies by means of free association. Here I feel that Mrs Brunswik does not stress sufficiently the fundamental rôle free association plays in psycho-analytic theory for providing the material to be interpreted. For every science has its own procedures for collecting the data relevant to

its theories. By this I mean not merely that the laboratory techniques and the mathematical-statistical tests differ according to the domain where they are employed; this is trivial. I mean that our theories must be such that we can specify, in some way, what counts as data for them; and this is a mainly semantic difficulty, for we must be able to give a plausible interpretation of the evidence we have found. Certain pseudo-scientific theories fail to convince us because, apart from other difficulties, we cannot see how the evidence—carefully collected and statistically ‘sound’ as it may be—does relate to the hypothesis. The data for our theories do not grow, like flowers, ready to be plucked; and it is unfortunately not true that we merely have to go and find the place where they grow. Theory, procedure, and evidence have to fit each other; and, though in a simpler theory like Newtonian mechanics it is in practice not difficult to satisfy this condition, this is obviously not so in a more ‘abstract’ theory, e.g. in quantum mechanics. The same holds for psycho-analytic theory. Once more, this is not the place to argue more fully about this problem. But it has to be faced by the methodologist: until now, most philosophers have taken the easy way out by assuming that nature provides ready-made data for all and every theory.

In the chapter that follows Mrs Brunswik discusses the psychodynamics of perception and cognition and points out that academic psychology is ‘cognition-centred’. Indeed, the strange doctrines about these processes which were first formulated some two thousand years ago continue to plague us. Philosophers have been too reluctant in giving up their various epistemologies; but is it not high time to let some fresh air blow them away? It may be true that intellectual processes such as perception, learning, and thinking are *partially* independent of unconscious motives and mechanisms; they are not completely independent of them. And even if they were, it is hardly useful to describe the working of the conscious mind in terms of Platonic ideas, Aristotelian universals, Lockean sensations, Kantian categories, or Cambridge sense-data. Even the academic psychologists of today do not speak like this. Is it so difficult to acknowledge that the grammar of words like ‘perceive’ or ‘know’ must fit in with modern psychology, if they are to be of use?

The final section of this monograph is about the relation of psycho-analysis to ethics. During the last fifty years Freud’s teachings certainly have had some effect on moral practice, at least in the western world. But few people have so far recognised the import psycho-analysis has for the philosophising about ethics, this is, for the theory of ethics. The so-called emotive theory current today has done excellent service in debunking old doctrines; but it has remained curiously negative. The reason, I think, is this. In ethics as little as anywhere else is it possible to forget the facts and to be content with discussing the use people make of value-words;

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for the use of our words is based upon, and refers to, what people actually feel, think, and do. We cannot explain how we employ 'good' and 'bad', 'right' and 'wrong', merely by considering the grammar of these words or, even, by appeal to common sense: the theory of psychology is essential for any explanation. If it is not brought in, we are in danger of using pseudo-facts for solving pseudo-problems, as philosophers so often did when they wrote treatises on the Nature of man or on the Passions. To cut ourselves off from scientific evidence even when only theorising about science or ethics is as useless as if we did so when working in the laboratory or when acting morally.

There are three general comments with which I would like to close. Mrs Brunswik, quite rightly I think, points out that 'some more formal attempts in the direction of an axiomatisation of the psycho-analytic system would be . . . useful'. Unfortunately, she has not made this attempt. Certainly it is easier said than done. It is, however, vital to try to separate what are the basic assumptions and laws and what are the hypotheses derived from them. One thing to remember here is that psycho-analysis consists of many theories. Too often people speak of *the* theory of psycho-analysis, as if there existed a unified and logically coherent account; in fact, we have a theory of infantile sexuality, a theory of dream interpretation, etc. Though all these theories are closely connected, it would make the methodological task less formidable if we were to discuss, in the first instance, a particular and limited theory. We should not have advanced very far in physics if we had tried to explain all of its theories in one swoop. Only after having established some sort of logical order within a theory can we succeed in showing what a psycho-analytic hypothesis is; and then we can proceed to discuss the criteria for its confirmation.

My next comment is this. Methodology is usually regarded as an antidote to traditional philosophy, that is, as a technical discussion (in terms of logic and semantics) arising out of the actual theory and practice of science. Even then we must take care not to fall back into the various -isms of epistemology since they introduce ontological commitments which are difficult to abandon afterwards; for these are usually not openly expressed. It is the more important in psychology since ontologies introduce a prior decision about the mind-body problem, and this we must try to avoid here at all costs. The empiricist type of argument which is occasionally employed by the author is therefore not very helpful.

This brings me to my last comment, namely, to point out the care Freud took to escape from the philosophical entanglements of the mind-body problem. Mrs Brunswik says that 'he speaks of the "insoluble difficulties" of psychophysical parallelism, or of interactionism for that matter, and often prefers to talk about the psychological vs. the physiological "language" . . . rather than about causal relationships between two metaphysically

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distinct systems' (p. 292). This need for caution, alas, has not been understood by many people even today.

Engaging in methodology is often avoided by the practical scientist, and sometimes for good reasons. With the progress of science, however, it becomes necessary not only to create new theories but also to explain them, that is, to speak about the main ideas and concepts contained in them. Mrs Brunswik's monograph is, as far as I know, the first attempt of a systematic and comprehensive discussion of psycho-analytic methodology. If I have offered some criticism, it must not be taken as detracting from the great merit of her book.

ERNEST H. HUTTEN

ANNOUNCEMENT

PHILOSOPHY OF SCIENCE GROUP (NORTHERN BRANCH)

ARRANGEMENTS are being made for a meeting at University College, Durham, on Saturday and Sunday, July 2 and 3. It will take the form of a symposium entitled :

Perception, Reception and Control

Provisional arrangements, subject to alteration, are :

SATURDAY (Evening) *Perception*

Professor F. V. Smith (Dept. of Psychology, University of Durham)

Mr I. P. Howard (Dept. of Psychology, University of Durham)

Mr Henri Tajfel (Dept. of Psychology, University of Durham)

Mr N. C. Loveless (Dept. of Industrial Health, Medical School, Newcastle-on-Tyne.)

SUNDAY *Reception and Control*

Dr A. M. Uttley (Ministry of Supply, Radar Research Establishment, Malvern)

Dr W. T. Catton (Dept. of Physiology, Medical School, Newcastle-on-Tyne)

Mr W. Sluckin (Dept. of Psychology, University of Durham)

Dr W. Mays (Dept. of Philosophy, University of Manchester)

Dr D. F. Cole (Dept. of Medicine, Medical School, Newcastle-on-Tyne)

Mr J. Meredith (Dept. of Electrical Engineering, King's College, Newcastle-on-Tyne)

Dr J. O. Wisdom (Dept. of Logic and Scientific Method, London School of Economics)

Titles of contributions are to be announced later.

Accommodation and meals will be available at University College, Durham Castle, Durham. Those wishing to stay at University College should make their arrangements through Dr W. Mays, Dept. of Philosophy, The University, Manchester, 13. Other questions should be addressed to Dr D. F. Cole, Dept. of Medicine, Medical School, Newcastle-on-Tyne 1.